THURSDAY, JULY 12, 1888.

ELECTRICITY AND MAGNETISM.

A Treatise on Electricity and Magnetism. By E. Mascart and J. Joubert. Translated by E. Atkinson. Vol. II. (London: De La Rue and Co., 1888.)

THE English translation of the second volume of the valuable work of MM. Mascart and Joubert is a welcome addition to the class, none too large, of really substantial English books on electricity. We have already directed the attention of the readers of NATURE to the first volume of this work; and we took occasion to point out that in their exposition of the subject the authors follow very closely the general methods of Clerk-Maxwell. That they do so is a great advantage for the English student; because it enables him, without breach in the continuity of his studies, to use Mascart and Joubert as a commentary upon Maxwell, who is often by no means easy reading. The French work is also supplementary to Maxwell, for writers avoid as much as possible the purely theoretical side of electrical science, and treat electrical phenomena, more especially in their second volume, as subjects of observation, and, above all, of measurement. This volume, which is now before us, is, in fact, an epitome of all the wisdom in exact electrical measurement which has been gained during a period of extraordinary activity in that field. This period began with the researches of Gauss and Weber; and may perhaps be said to have culminated in the great series of determinations of the absolute units which were made about the time of the Congress of Electricians at Paris in 1884. The prominent part taken by MM. Mascart and Joubert in this work has well fitted them to record with precision the details of the leading methods by which it was accomplished, and it would be hard to refer the student of electrical science to an authority on electrical measurement at once so clear and precise in detail, and, with a few small and evidently accidental exceptions, so manifestly candid and fair, as the second volume of the treatise of Mascart and Joubert.

The space at our disposal in the pages of NATURE allows us to give but a brief summary of the contents of this volume. Part I. deals with the auxiliaries of electrical measurement, such as the measurement of angles, of the periods and amplitudes of oscillations, of couples, and of such properties of circular currents as are important in the construction of galvanometers and other electrical instruments. As an example of the care with which the subject is treated, we may refer to the discussion, in §§ 659, 660, of the power of a telescope, and of the relation that ought to subsist between the dimensions of a graduated circle and of the telescope with which it is associated. The conclusion of this discussion is marred in the English version by inadequate translation. Thus, for example, "un cercle de ce diamètre [80 cm.] devra donc être associé à une lunette de 16 centimètres d'ouverture," does not mean "a circle of this diameter is therefore comparable with a telescope of 16 cm. aperture." The meaning is, that, to get the full use of the circle, a telescope having an objective lens of 80 cm. aperture is required; and that a more powerful one is unnecessary.

Part II., which is the kernel of the volume, describes the various electrical measurements as they are carried out in practice. There are chapters on electrometry, and on measurement of current, resistance, electromotive force, capacity, constants of coils, absolute resistance, and the fundamental velocity, *v*. The methods are described in great variety and with great detail. They are illustrated by giving not only the old classical results, but also by means of the most recent examples. Nothing is attempted like the exhaustive catalogue of results, good, bad, and indifferent, which makes Wiedemann such an invaluable book of reference. Experimental results are given simply as part of the exposition of the methods by which they are obtained. It is probably for this reason that the authors make no mention of the valuable experiments on dieletric strength recently made by their fellow-countryman, Baille.

Part III. is devoted to magnetic measurements, and is excellent so far as it goes. It is by no means so exhaustive as the purely electrical part; and, probably for that very reason, will be found to be lighter reading for the tyro in electricity and magnetism; to such we commend more especially the parts relating to the determination of so-called magnetic poles and to the magnetism of feebly magnetic and diamagnetic bodies, subjects which are very frequently imperfectly understood or inadequately expounded in current text-books.

Part IV., which is called a complement, deals with industrial applications, and contains a table of numerical constants. The table of constants gives full references to the sources of such information as it contains, and will be found most useful. The part that deals with industrial applications is to our thinking the least satisfactory part of the book; not because there is any want of clearness or soundness in it, but because it is too short and too scantily illustrated by references to practical cases to give the student any real idea of the problems that surround the electrical engineer.

In describing the various methods of electrical measurement the authors are, on the whole, very sparing of criticism. They seem to assume that they are addressing an audience fitted to draw their own conclusions from the facts put before them. Occasionally the weak points of the methods used by various experimenters are pointed out, but the authors never indulge in that species of criticism which consists in treating a fellow-labourer and all his productions with indiscriminate scorn because the critic has discovered some microscopic oversight, or believes that he has wrung one more decimal place from reluctant Nature.

There are one or two little points which might be amended in a future edition. For example, the elegant method of discussing resisted motion by means of the equiangular spiral, given in § 682, should be attributed to its author, Prof. Tait. The use of the fish-back galvanometer-needle (*i.e.* a needle made up of a number of separate parallel needles) was not an invention of M. Deprez, at least not an original invention; for the writer used, more than twelve years ago, a galvanometer fitted with a needle of this sort, which had been constructed for the B.A. Committee of 1867. Who the inventor was, is doubtful; but probably he took his idea from the laminated magnets constructed by Jamin and others. Perhaps the most serious historical oversight is made in § 1274, where, in

speaking of the graphical characteristic of a dynamo, language is used which would lead the reader to infer that this important method in the theory and practice of electrical engineering was introduced by M. Marcel Deprez, the fact being that it was first introduced, fully explained, and actually used by Dr. Hopkinson in 1879.[1] What M. Deprez did, was, we believe, simply to give a name to Hopkinson's curve, and to further develop its applications. It would be easy to correct, in footnotes or otherwise, these and a few similar small blots on a work which is, in most respects, remarkably fair and cosmopolitan in its history.

Regarding the work of the translator, we can, on the whole, speak very favourably. There are, however, passages here and there which are so inadequately translated that they suggest the idea of an inferior assistant not always sufficiently overlooked. Compare, for example, the following piece of the original with the accompanying translation:—

"Si la loi était générale, on en conclurait, pour le cas de deux plateaux parallèles, que la production de l'étincelle correspond toujours à une même valeur de la densité électrique et, par suite, de la force électrique et de la pression électrostatique, ou, dans les idées de Maxwell, à un même état ou une même énergie spécifique du milieu interposé."

"If the law was general, we should conclude, for the case of two parallel plates, that the production of electricity almost represents the same value of the electrical density, and therefore of the electrical force and the electrostatic pressure, or, as in Maxwell's views, to the same condition or the same specific energy of the interposed medium."

It will be seen that the English passage is not a translation of the French, is not English, and means nothing. We mention this, by far the worst, case of loose translation that we have noticed, to draw the attention of the English editor to the need there is for revision. Such corrections as are absolutely necessary might be given on a fly-leaf; and, in order to help, we mention a few things that we have noticed. Some are misprints, some wrong, some merely doubtful.

P. 39, "compass of horizontal intensity"?

P. 41, "observations" (oscillations?)

P. 48, "collate" (collect?)

P. 50, "bodies of easy construction"?

P. 237, "combine the experiment"?

P. 293, "but they are not sufficiently so, &c."?

P. 557, "*residues* of the Leyden jar"?

P. 577, "induced *charges*" (décharges induites)?

P. 578, "to *make the constant* of the ballastic galvanometer" (faire la tare: why use tare? Tare is English).

P. 878, "regulation of a galvanometer" (tarage d'un galvanomètre)?

Notwithstanding minor shortcomings, this English translation of the work of MM. Mascart and Joubert will be of great use to English readers; and we hope that it will not be thought that, by calling attention to inaccuracies here and there, we mean to depreciate the labour of the editor, or to undervalue the debt which the English scientific public owes him for rendering more accessible one of the most important electrical treatises of the day.

C. C.

[1] See his papers in the Proceedings of the Institution of Mechanical Engineers, April 1879 and April 1880.

SYNOPTICAL FLORA OF NORTH AMERICA.

Synoptical Flora of North America: the Gamopetalæ. A Second Edition of Vol. I. Part 2, and Vol. II. Part 1, collected. By Asa Gray, LL.D. Large 8vo. 480 + 494 pp. (Washington: Published by the Smithsonian Institution, 1888.)

THE first feeling which the sight of this book reawakens in the mind is one of deep regret that Prof. Asa Gray did not live to carry out the plans he had entertained so long for an elaboration of a complete flora of Temperate North America upon one uniform plan. A work of this scope was planned by Dr. Torrey and himself when he was quite a young man, and the first part appeared as long ago as 1838. It was soon found by the authors that it was impossible to identify satisfactorily the plants which had been named by their predecessors without studying the European Herbaria; and in order to do this Dr. Gray spent a year in Europe in 1838-39. Another instalment, which extended to the end of Polypetalæ, was published in 1840, and the remainder of the first volume, extending to the end of Compositæ, in 1842. Then Dr. Gray accepted the post of Fisher Professor of Natural History in the University of Harvard, and what with teaching and herbarium work, and the preparation of the successive five editions of his "Flora of the Northern United States," and the elaboration of the new collections that poured in as fresh territories were explored and settled, his time was fully occupied for thirty-five years. In 1878 he returned to the more comprehensive work, and in that year published the first part of the second volume, which includes the remaining orders of Gamopetalæ, from Goodeniaceæ to Plantaginaceæ. In 1884 he issued a revised edition of the part devoted to the Compositæ and small allied orders. The work we have now before us is a reprint of the whole of the Gamopetalæ, with two supplements, embodying additions and corrections up to the end of 1885. Although the title-page bears the date of 1888, it was really issued, as the secondary title-page indicates, in January 1886, and we have had it in use at Kew for a couple of years. The present volume, therefore, covers the central third, brought up to date, of the complete undertaking as planned; and at the beginning the Polypetalous Dicotyledons are still left as they stood in 1840, except for the most useful bibliographical index, brought up to date, which Dr. Sereno Watson issued in 1878; and the Incompletæ and Monocotyledons, to which Dr. Watson has happily devoted special attention during many years, have still to be dealt with.

The flora of Temperate North America contains about the same number of species as that of the whole of Europe, but of course the orders are to a certain extent different, and others enter in the two floras in very different proportions. In the present work there are described 3521 species of Gamopetalous Dicotyledons, of which all but 162 are indigenous. They fall under 562 genera, of which 520 are native. The American Compositæ alone, 1636 species, far more than outnumber the whole Phanerogamic flora of Britain. Next to Compositæ come Scrophulariaceæ, represented by 367 species and 38 genera. Of Hydrophyllaceæ, an order nearly restricted to North America, there are 129 species and 14 genera; of Polemoniaceæ, another nearly endemic

order, there are 133 species. The more tropical character of the North American, as compared with the European, flora, is shown by the presence of 44 non-stellate Rubiaceæ, 9 Sapotaceæ, 97 Asclepiadaceæ, 6 Bignoniaceæ, and 41 Acanthaceæ. To get such a large number of plants worked up by such a model systematist as Dr. Gray is an enormous boon to all species botanists. A great many of the species are here described for the first time; and a still larger proportion have only been previously noticed in scattered unclassified papers. A large number of the best-known North American plants cultivated in our gardens belong to Gamopetalæ; and to have such genera as Aster, Solidago, Helianthus, Pentstemon, Phacelia, and Gilia, put in order and brought up to date will be a great saving of time and trouble, and make the book essential, not only to botanists, but to all owners of gardens who wish to understand the characters, affinities, and geographical distribution of the plants they grow.

In arranging their material the authors of the four great recent descriptive local floras have followed four different plans. In Bentham's "Flora Australiensis" there is, under each genus, an initial analytical key, in which each species is distinguished, and afterwards a single detailed description of each species and its varieties. The 8500 species of the Australian flora, described after this plan, fill seven volumes of from 500 to 800 pages each. In Boissier's "Flora Orientalis" the initial key only goes down to the sections, and there is a less detailed single description given of each species. The number of species is about 10,000, and the whole work runs on to five large volumes of about 1000 pages each. In Sir J. D. Hooker's "Flora Indica" there is no initial key, but sub-genera and groups are briefly characterized, and under each species is given both a compact diagnosis and brief description. Under this plan the 10,000 Dicotyledons of India fill five octavo volumes of 700 to 800 pages each. Dr. Gray gives no initial key, more detailed characters of sub-genera and groups, and under each species a single short description. Under this plan the 3500 Gamopetalæ fill a book of 970 larger pages. It is an omission, we think, that Dr. Gray has not numbered his species, for, in referring from the book to the herbarium and back again, such numbers are a very useful guide. Mr. Bentham, Sir J. D. Hooker, and Dr. Gray all three adopt the same comprehensive idea of what constitutes a species, and use substantially the same orders and genera, and the same plan of nomenclature; and it is a very great convenience in herbarium work that these three great floras have been treated upon one uniform system.

Our best sympathies are with the American botanists in the great loss they have sustained. In securing such a competent assistant as Dr. Sereno Watson, Dr. Gray was very fortunate, and we trust that the material for the two other volumes is in such an advanced state of preparation that they may be published under his editorship before long. We European botanists have great reason to thank the managers of the Smithsonian Institution for their liberality in granting funds for the book. What a boon it would be if we could have a general flora of Europe planned upon the same lines; but with all our great Universities and Herbaria this does not at present seem at all likely.

J. G. BAKER.

HYDRODYNAMICS.

Treatise on Hydrodynamics. Vol. I. By A. B. Basset. (Cambridge: Deighton, Bell, and Co. London: George Bell and Sons. 1888.)

THIS book deserves to be most warmly received by all who are interested in this branch of mathematics, in which remarkably rapid progress has been made of late years. For some time past a constant and familiar acquaintance with the Proceedings of learned Societies has been necessary to enable students to keep abreast with the subject; and the author has performed real service in incorporating in his work many important results and memoirs.

This volume, which is to be followed by a second, contains the general equations of motion, with the auxiliary discussions of vortex and irrotational motion, and also the theory of motion of solids in a fluid, in which both the hydrodynamical and the dynamical effects of the motion are very fully discussed. The chapter on the equations of motion is noticeable for the introduction of Clebsch's transformation, proving the permanency of vortex lines and vortex sheets, and for the application of the principles of least action and energy. Students are apt to lose sight of general dynamical principles in the not inconsiderable difficulties of pure analysis that attend this subject; and it is well they should be aided to bear in mind that their symbols are after all intended to represent physical phenomena, while it adds considerably to the interest of the subject to exhibit its analogies with kindred physical principles. But it is to be regretted that in this chapter the author has not removed the obscurity which arises from the fact that the equations of motion can be obtained in the same form by either a Lagrangian or an Eulerian method. The device of endowing each particle of fluid with co-ordinate axes, all its own, marks the first method; the observation of fluxes at a certain point of space is the distinguishing feature of the second. To identify the results thus obtained (as *e.g.* in the equations of motion in spherical co-ordinates) is justifiable, but will certainly lead to much misapprehension at the outset.

A chapter on images and doublets is useful as collecting together what must otherwise be introduced in a random manner. The discussion of motion in two dimensions is as complete as the limited number of cases that are soluble will allow; several new cases, not previously found in the text-books, illustrate the increasing difficulty of the analysis. In dealing with discontinuous motions the author follows Kirchhoff, beyond whose work on this question it seems impossible to advance.

The second half of the book, treating of the motion of solids in a fluid, is singularly interesting, and contains the last contributions to the dynamical theory, which are due to the author himself. These are marked by great generality of treatment and power of analysis, but we fear the complexity of the results will prevent their being generally appreciated.

In dealing with the velocity-potential due to the motion of an ellipsoid, it would appear that the most direct and general method of obtaining the result in every case is to form Laplace's equation in ellipsoidal co-ordinates; instead of this the author has recourse to formulæ in the theory of attraction, which need modification to suit each special case.

A chapter on the motion of two spheres indicates the attention given to this problem of late years, and may also serve as a warning of its hopelessness. The anticipations of its yielding an explanation of magnetic phenomena, to which the first experiments by Bjerknes gave rise, have been dissipated by the exhaustive mathematical treatment it has received.

The excellence of this work leads us to look forward with great interest to the publication of the second volume, which will deal with fresher and more suggestive portions of the subject; and the two volumes together will prove of very great use to every student. The words on the title-page, "with numerous examples," strike us as below the dignity of a subject like hydrodynamics. The book will certainly be appreciated for its own merits even more than for its examination usefulness, to which aim too many books conform.

OUR BOOK SHELF.

Sierra Leone; or, the White Man's Grave. By G. A. Lethbridge Banbury. (London: Swan Sonnenschein, Lowrey, and Co., 1888.)

THE author of this book explains that he does not offer it as "one of travel over unknown ground" or "as one of dangerous adventures and hardships." His aim simply is to bring before his readers a description of an Englishman's life in "the most interesting but deadly colony of Sierra Leone." He has done his work well, and the book will be cordially welcomed by all who have any special reason for wishing to obtain clear and accurate information about this part of the West African coast. The volume consists chiefly of letters written while Mr. Banbury was at Sierra Leone, and has therefore a freshness and vividness which it would have been hard for him to match in a more elaborate and formal work. The most valuable chapters are those in which he sets forth the impressions produced upon him by the natives, in whose ideas and customs, as here depicted, there is an odd mixture of Christianity and the lowest forms of paganism. Mr. Banbury has a strong belief in the power of education to improve the character of the native population, and he urges that more strenuous efforts should be made for the establishment of proper schools. It is tolerably certain that if permanent good cannot be done to the colony by this means there is no other way in which real progress can be secured, for, as Mr. Banbury points out, the unhealthiness of the climate prevents any large increase of the number of European settlers.

Nature's Fairy-Land: Rambles by Woodland, Meadow, Stream, and Shore. By H. W. S. Worsley-Benison. (London: Elliot Stock, 1888.)

THIS book consists of a series of papers selected from a considerable number which have appeared in various periodicals. The author has a clear, pleasant style, and his vivid descriptions and explanations are well adapted to awaken in the minds of young readers a genuine interest in various aspects of scientific truth. The volume opens with an attractive paper on "The Journeyings of the Rain Drops," and this is followed by papers entitled "From Root to Flower," "Out Among the Gorse," and "Companions of the Corn." These three papers serve as an introduction to other chapters on plant-life. There are also interesting essays on such subjects as shells and shell-builders, spiders, and the nests of fishes.

Lessons in Elementary Mechanics. By W. H. Grieve, P.S.A. (London: Longmans, Green and Co., 1888.)

THE second stage of mechanics is alone dealt with here, and throughout, the author has rendered the various forces which produce motion, together with the laws which regulate those forces, in a clear and simple style; the illustrations are numerous, and are specially adapted to an elementary course. The work is suited to the requirements of the second stage of the revised code, and the arrangement of the chapters is the same as that in the Syllabus of Instruction adopted by the London School Board. The examples at the end of each chapter are instructive and well chosen, and the book concludes with a series of examination papers and results to the numerical questions.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Photography of Lightning.

So much interest is being taken at the present time in obtaining photographs of lightning flashes, that perhaps some one would be willing to take the necessary trouble, and use a moving camera. A camera revolving or vibrating at any ordinary pace would probably give each single flash unaltered, but it might analyse multiple and complex flashes into their constituents.

The eye is so easily deceived as to what is really happening in these sudden effects that very erroneous views may easily be formed, and indeed are in some quarters now prevalent.

Whether it is better to make the camera revolve as a whole, or only the sensitive plate, or whether a revolving mirror should be used with a stationary camera, are questions for experience to decide.

One good method, if not too troublesome in practice, would be to arrange a double camera, with component axes parallel, so as to photograph the same flash in both halves, but with the sensitive plate of one fixed, of the other rapidly revolving. Appearances really due to succession in time could be then easily distinguished, and might be capable of interpretation.

July 10. OLIVER J. LODGE.

Micromillimetre.

I AM glad that the Council and Fellows of the Royal Microscopical Society have seen their way to the adoption of the word *micron*, but the letter in which Mr. Crisp announces this decision to you is not, I think, calculated to give a correct impression of the circumstances under which it was taken.

Firstly, I need hardly say that I did not take exception to the word *micromillimetre*, but to its use as equivalent to the thousandth of a millimetre.

In the next place, I wrote to the Secretaries of the Royal Microscopical Society on behalf of the Council of the Physical Society, of which no mention is made by Mr. Crisp.

Thirdly, the proposal of the Council of the Physical Society was that the word *micron* should be adopted.

Lastly, I am myself quite in favour of this course, and in fact moved its adoption by the Physical Society.

The word *micromètre* must in accordance with the rules of the B.A. Committee be a possible alternative just as a *cubic decimetre* is alternative to a *litre*, and I think the disadvantage of the multiplication of special names not based on a uniform system is nearly as great as that of the possible occasional comparison between micromètre and micromèter. This is however a little matter as compared with the use of *micromillimetre* in two different senses, and the official sanction of *micron* by the French authorities (of which I was not aware when I originally wrote to you) is quite sufficient to turn the scale in its favour.

As some of your readers may not have seen the previous correspondence, will you allow me in conclusion to state that it is now generally agreed,

(1) That the thousandth of a millimetre shall be called a *micron* and denoted by μ.

(2) That the millionth of a millimetre shall be called the *micromillimetre* and denoted by $\mu\mu$.

ARTHUR W. RÜCKER.

Distribution of Animals and Plants by Ocean Currents.

I BEG to forward you herewith some extracts from a letter just received from Port Elizabeth, South Africa, which, I think, cannot fail to interest your readers in connection with Darwin's theory of the distribution of animals and plants in some cases by ocean currents. My correspondent writes :—

"About the beginning of the year 1887 the attention of the public of Port Elizabeth was aroused by finding a quantity of pumice-stone washed up upon the shores of the bay, showing volcanic action. Some of the pieces were covered with barnacles of a few months' growth, and others appeared as though a mass of vitrified matter had been poured upon them. At the same time, shipmasters stated that they had seen large masses floating upon the sea as they approached the east coast of Africa. Strange fish also made their appearance in our waters, and, among the number, two large specimens of the ox-ray species were found washed up upon the rocks. But more remarkable was the discovery of four venomous sea-snakes about 18 inches long, the bodies marked black above and yellow below, answering the description of the *Pelamis bicolor* usually to be found about the coasts of Sumatra, Java, and the adjacent isles, and which must have followed the floating debris. One of these snakes was still alive when found, although it did not long survive, and one of the others was in a sufficient state of preservation to be sent to the Museum. What will prove more interesting still, is the discovery of a large seed resembling a cocoa-nut, which was picked up about the same time, of which Mr. Russell Hallack, of Port Elizabeth, gives the following description :—

"'About the latter end of 1886 a large husky fruit was picked up. It resembled a square cocoa-nut of 4 inches cube, not quite so deep as broad and long. Inside this husk, which was more cork-like than fibrous, was a solitary nut, about 1¾ inch round, melon-shaped, with fluted outside, covered with a coating resembling potato-peel. This nut had been bitten by the boy who found it, but whether the taste was not to his liking, or for some other reason, he was persuaded to give the remains to the gardener of the north-end park, who planted it. In due time the shoot came up like a potato-plant with small leaves. The plant is now about 4 feet high, and the small leaves have developed into grand foliage 20 inches long by 7 or 8 broad. It is supposed to be the *Barringtonia speciosa*, a native of the East Indies. A smaller variety, the *B. racemosa*, is said to exist in Natal and the east coast of Africa, but is easily distinguished from this by the smallness of its fruit. The *B. speciosa* belongs to the myrtle tribe, but differs from the ordinary type in having this large, one-sided, corky husked fruit; it is one of the handsomest of its tribe, and in the Moluccas attains the height of 40 or 50 feet, with a circumference of 10 to 14 feet. It is generally found near the sea.'"

The suggestion is that this nut, as well as the snakes, the strange fish, and the pumice-stone, are all relics of the great Krakatão eruption in 1883, and that they had drifted about till the beginning of 1887, till thrown upon the coast of South Africa. If this be really the case, the tenacity of life in the snakes and the nut is truly remarkable, and, as my correspondent adds: "Surely some of this debris must have been deposited on the island shores visited by these currents, and if we could only become acquainted with the date of their appearance upon each, some idea might be formed as to the course taken by these plants, &c., in their journey to Southern Africa."

I find, by a reference to the back numbers of NATURE, that the pumice has been traced to the east coast of Africa, leaving portions on various islands *en route*, and that some of it was timed to reach the west coast of America at Panama in 1886; but nowhere do I find any notice, except that given above, of animal or vegetable debris accompanying the masses of pumice. Perhaps the publication of these interesting facts may call forth similar observations from some of the Pacific Islands.

A. W. BUCKLAND.

Watches and the Weather.

MY neighbours, Messrs. Jacob and Ross, watchmakers, often tell me their experiences in the breaking of mainsprings.

Unreflecting people fancy they have broken the spring by over-winding, or in other words have drawn asunder a piece of steel by the force of finger and thumb.

The springs of course break through a subtle molecular change produced in the steel by atmospheric causes: they usually fly asunder a few hours after being wound, at 3 or 4 o'clock in the morning. Many watches and clocks come to the workshops for new springs after a frost, but not until a thaw has set in; still more come after thunderstorms.

This morning a clock spring was taken out of its box, which had overstrained itself at one moment into seventeen pieces, there was a complete fracture in each coil along a radial line from the centre. Some time back one was found with three such radial lines of fracture.

Of course this subject is not new, but it gains by recorded experiences. W. B. CROFT.

The College, Winchester, July 9.

Preserving the Colour of Flowers.

I SHOULD be greatly obliged if some of your readers would inform me how to preserve the colour of those flowers prone to fade during and after pressing.

In a local paper I saw an extract from the *Pharmaceutical Journal*, in which salicylic acid was recommended. I have tried it both as powder and in solution in spirit; in either case it had a great tendency—except in the case of yellow flowers—to change the colour to either a bright scarlet or to a light brown. A. W.

[There is no difficulty in preserving the colour of yellow flowers if they are properly dried by the ordinary method, *i.e.* in absorbent paper, changed at the end of the first day, and once or twice afterwards. It is very difficult to prevent such plants as Pedicularis, Bartsia, and Melampyrum turning black. See an account of a plan recently tried in Germany by Schönland, in *Annals of Botany*, vol. i. p. 178, 1887.—J. G. BAKER.]

THE LIFE STATISTICS OF AN INDIAN PROVINCE.

SOME years ago, in this journal (vol. xxix. p. 338), I published a short article on the intimate relations which subsist between meteorological conditions and the statistics of death and crime in India. In this it was incidentally mentioned that, imperfect as they were, the vital statistics of the North-West Provinces and Oudh were at that time more to be depended on than those of any other province in India, thanks to the unremitting attention paid to the subject of registration by the late Sanitary Commissioner, Dr. Planck; and though they have not sensibly improved since 1884, but perhaps rather fallen off in accuracy, the birth and death registers of these provinces are still undoubtedly better than any others in India embracing an equal population.

As ten complete years have now elapsed since the amalgamation of the two provinces, which together contain a larger population than any European country except Russia, and as similar statistics are not at present obtainable from any other Oriental country but India, it may be of interest to compare some of the conditions of life revealed by them with those obtaining in the more favoured countries of the West. That India has a high death-rate, owing to the unhealthiness of the prevailing climatic conditions and imperfect sanitation, as well as to the low vitality of the mass of the people consequent upon superabundant population and insufficient food, is universally understood; but there is no proper appreciation of the marvellous recuperative power of a population among whom prudential restraints on increase are unknown, and where almost every woman has been married in childhood, and commences to bear children at the age of fourteen or fifteen years. It may be said with almost absolute truth that there are not only no old maids in India, but no unmarried women above the age of puberty, except the unfortunate class of Hindu widows of the higher castes, who are not permitted to marry again; but though this class appeals in many ways to our sympathies, it is of very slight importance from the point of view of the increase of population, the widows of child-bearing age amounting to only 9 per cent. of the

total number of females of the same age—a proportion which compares very favourably with that of widows and spinsters in England. This wonderful power of rapid recovery after decimation by famine or pestilence will be fully exhibited in the tables given below.

The registration of deaths was in regular operation for several years, both in the North-West Provinces and in Oudh, before the two were united under one administration in 1877. That of births was first introduced generally in 1879, though it had been tentatively commenced in municipalities and cantonments some time previously. We have therefore now (February 1888) ten complete years' death statistics for the united provinces, of a fairly uniform degree of accuracy, and nine years' registers of births, decidedly improving in completeness and accuracy for the first five or six years. The births for the first year of the ten—1878—may also be approximately arrived at by a proportionate computation from those registered that year in municipalities. The total number of births of each sex, registered in each of the ten years, was as follows :—

Year.	Males.	Females.	No. of Males to 100 Females.
1878	667,975*	545,285*	122·50
1879	669,921	555,911	120·51
1880	747,953	642,826	116·34
1881	948,191	831,282	114·06
1882	875,616	780,543	112·18
1883	950,932	850,469	111·81
1884	1,015,699	915,262	110·98
1885	957,672	861,609	111·15
1886	874,099	785,433	111·28
1887	902,844	805,891	112·03
Total	8,610,902	7,574,511	113·68

An inspection of the last column shows that these numbers require to be corrected, not only by an allowance for general incompleteness of the records, but by a special addition to counteract the tendency to omit females. During the first seven years this tendency diminished as registration improved, and the numbers of the two sexes approximated more and more to equality; but even with the most intelligent and careful recording agency, the true ratio between the sexes at birth will never be attained in the records until the opinion of the mass of the people on the relative values of male and female life has undergone a complete alteration. The ratios for the first seven or eight years in the table give a curve apparently asymptotic to a certain line, the ordinate of which would stand for the ratio attainable by the greatest care in registration under the present conditions. Representing the above ratios for the first eight years by the formula, $a + \frac{b}{t}$, where t is counted in years from 1877, we find the ordinate of the asymptote, a, to be 108·57. In the provinces there are, however, two districts in which the numbers born of the two sexes invariably approach much more nearly to equality. One is Garhwál, a Himalayan district inhabited by an unsophisticated people who claim to be *Rájpúts*, but are probably of aboriginal descent, and who have never come under Muhammadan influence in any way, or acquired the custom of paying a heavy dowry with the bride, which is the cause of female infanticide among many of the higher castes. The other is Lalitpur, in the extreme south, where the inhabitants are chiefly *Chamárs* and other low castes, who have never concealed their women or practised infanticide, and amongst many of whom the bridegroom's family pay for the bride. The statistics for these two districts give a series of ratios represented by a curve whose asymptote has an ordinate of 100·00, or which points ultimately to exact equality between the sexes. In like manner, if we select for each year that district in which the recorded birth-rate was highest, and where, therefore, the registration was presumably most complete, we get a curve pointing to an ultimate ratio of 102·78 males to 100 females. If we take the mean of all three results, that for Garhwál and Lalitpur being probably below the true average for the whole population, we get 103·78 males to 100 females. This comes very near the ratio for England, which, I believe, is between 103 and 104, and is almost identical with that deduced from the distribution of the population according to age and sex at the last two censuses of the North-West Provinces—namely, 103·75. It may therefore be adopted as a close approximation to the truth, and it shows that, in regard to the relative numbers of the sexes, human nature is much the same in the East and West, notwithstanding the deceptive appearance presented by unanalyzed statistics, as well as by public gatherings in countries where respectable women seldom venture out of doors.

* Estimated from those registered in municipalities.

The numbers of females in the above table must therefore be all recast so as to give 103·78 males for every 100 females.

This special inaccuracy in the birth tables being corrected, there remains the general inaccuracy due to incompleteness of the register, which is common to both births and deaths, and has been estimated by Dr. Planck, after careful and extended personal inquiry, at 20 per cent. of the total, or one-fourth of the numbers recorded. When both causes of error are allowed for, the total number of births in each year will be as in the second column of the next table. The third column gives the recorded deaths, increased by 25 per cent. to make them represent approximately the true mortality, and the last shows the increase or decrease of population each year, due to these causes. The figures in this column represent very fairly the total gain or loss of population, for the number of emigrants is only three or four thousand annually, and this loss is partly balanced by a return migration, the numbers of which are not known.

Year.	Births.	Deaths.	Increase.
1878	1,639,544	1,902,175	− 262,631
1879	1,644,320	2,393,124	− 748,804
1880	1,835,850	1,601,544	+ 234,306
1881	2,327,335	1,753,091	+ 574,244
1882	2,149,219	1,856,409	+ 292,810
1883	2,334,062	1,520,371	+ 813,691
1884	2,493,036	1,944,177	+ 548,859
1885	2,350,606	1,763,299	+ 587,307
1886	2,145,476	1,834,516	+ 310,960
1887	2,216,030	1,977,174	+ 238,856
Total	21,135,478	18,545,880	+ 2,589,598

During the year of scarcity, 1878, and that of pestilence, 1879—for the great epidemic of unprecedentedly fatal malarial fever that year surely deserves the name of pestilence—the net loss of population was over a million; but in the next three years this was fully recovered, and in the succeeding years large numbers were added to the population, especially in the healthy year, 1883. Thus the net gain for the ten years, notwithstanding famine and pestilence, was over two millions and a half, an increase almost unprecedented since the first census in 1853, and doubtless the result of an unusually long succession of abundant harvests. Since 1885, however, the increase has grown less and less rapid; and as another scarcity is now nearly due, if any trust may be placed in the average period of the recurrence of droughts in the past, it seems likely that in the next two or three years the increase may be temporarily stopped.

With these figures, and the fixed point given by the census of 1881, it is possible to find the probable number living at the commencement of each year from 1878 to 1888, and also the mean birth- and death-rate for each year of the ten. The census was taken on the night of February 17, 1881, and the total of the people numbered

was 44,107,869. In the Census Report it is, however, shown that over a million females between the ages of 5 and 20 must have escaped enumeration; and when allowance is made for them, the probable accurate total comes out 45,232,391. During January and the first seventeen days of February the increase was 118,532; so at the beginning of 1881 the population stood at 45,113,859. From this starting-point the following figures have been worked out:—

Year.	Number living at Commencement.	Year.	Number living at Commencement.
1878	45,890,988	1884	46,794,604
1879	45,628,357	1885	47,343,463
1880	44,879,553	1886	47,930,770
1881	45,113,859	1887	48,241,730
1882	45,688,103	1888	48,480,586
1883	45,980,913		

The mean number living during the ten years was 46,478,714.

The total area of the united provinces is given in the Census Report as 106,104 square miles. The population is thus at the present time about 457 to the square mile, including in the average the Himalayan province of Kumaon, over 12,000 square miles in area, where the average density is less than 90 to the square mile. There is practically no export trade, except in agricultural produce; hence the whole population is supported directly or indirectly by the agriculture of the province; and there is probably no purely agricultural country in the world, except perhaps some parts of China, where so dense a population is maintained.

The birth- and death-rates and rate of increase or decrease each year, calculated on the usual basis of 1000 living, are given in the next table.

Year.	Birth-Rate.	Death-Rate.	Rate of Increase.
1878	35·83	41·57	− 5·74
1879	36·33	52·88	− 16·55
1880	40·54	35·37	+ 5·17
1881	51·26	38·61	+ 12·65
1882	46·89	40·50	+ 6·39
1883	50·32	32·78	+ 17·54
1884	52·97	41·31	+ 11·66
1885	49·35	37·02	+ 12·33
1886	44·72	38·24	+ 6·48
1887	45·82	40·88	+ 4·94
Mean	45·40	39·91	5·49

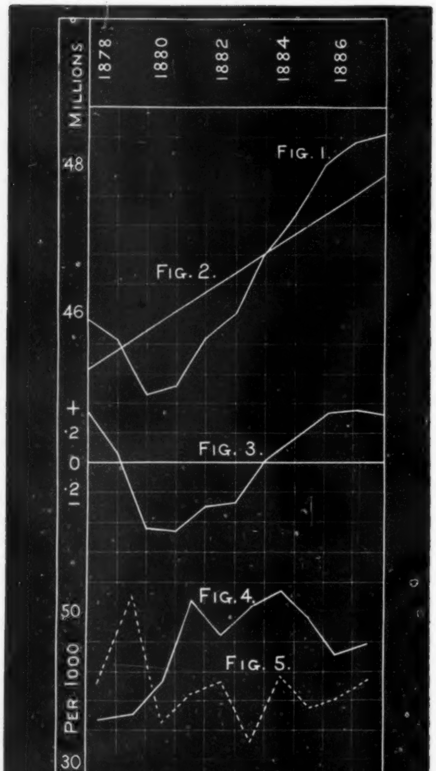
The birth-rate, even in the worst years of the ten, was as high as in England, while in the best years it was about 50 per cent. higher. The death-rate averaged nearly forty per mille, and therefore, notwithstanding the high birth-rate, the population increased only at the rate of 5·5 per thousand per annum.

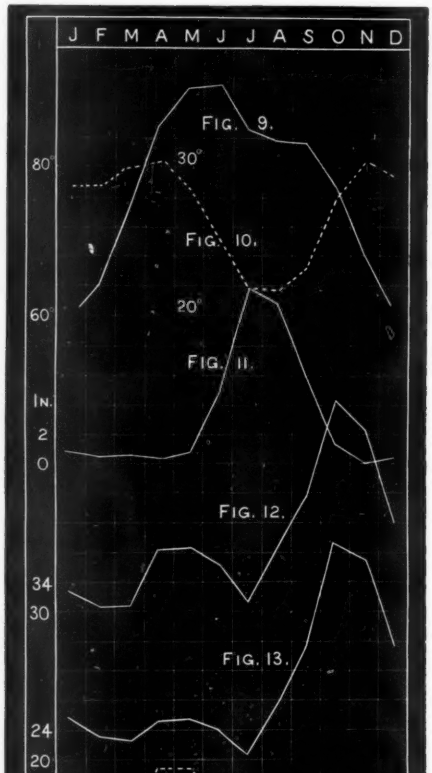
A glance at the annexed diagram will render these data more intelligible. Fig. 1 exhibits the movement of the total population from year to year; and the nearly straight line, marked Fig. 2, shows what this movement would have been had it proceeded uniformly at the rate of 5·49 per mille per annum. The curve has been computed by the formula $P = P_0 r_n$, where n is counted from the beginning of 1883, and consequently the ordinate for 1883, P_0, is the geometrical mean of all the ordinates of Fig. 1. The differences of the ordinates of the first two curves are charted in Fig. 3, which therefore exhibits the extent to which the actual population exceeds or falls short of that given by a uniform movement. This is apparently a periodic function of the time; and if so, the period does not differ much from ten years, since the last ordinate is only slightly greater than the first. Figs. 4 and 5 represent the birth- and death rates respectively. At first sight these appear to have no relation to each other, as concomitant and opposed variations are nearly equal in numbers. The years of lowest death-rate, 1880 and 1883, were, however, followed by the years of highest birth-rate, showing that healthy conditions conduce to fecundity as well as diminished mortality. An exception to this rule is, however, found in 1879, which, following a healthy year, should have had a high birth-rate, but was marked instead by an exceptionally low one. The year 1878, though a dry and very healthy year, was one in which the vitality of the people reached a low ebb, owing to long-continued scarcity, approaching in some places to famine; for, though few or none actually died of starvation, millions were for many months at starvation-point.

The annual rate of increase per thousand, shown in Fig. 6, is probably the best possible measure of the general well-being of the people, combining as it does the effects of abundance or scarcity of food, which influence the birth-rate, with those of health and disease, on which the death-rate depends. Curiously enough, this index of general prosperity or the reverse is much less liable to sudden fluctuations than the birth- or death-rate alone, and yet, like the numbers represented by Fig. 3, it is apparently subject to a periodic oscillation about a mean value. The length of the period is probably something over ten years, since the last year gives a considerably greater result than the first, though it exhibits a downward tendency. It is therefore possible that the rate of increase of a primitive people, living a natural life untrammelled by too much civilization, and multiplying up to the limit of the means of subsistence, may be subject—like the prices of grain, investigated by Mr. E. Chambers and the late Mr. Stanley Jevons, and like many other terrestrial phenomena—to a periodic variation determined by that of the energy received from the sun. Assuming that there is a variation with a period of eleven years, the rates of increase charted in Fig. 6 lead to the formula, $r = 4·576 + 11·725 \sin\left(\frac{2\pi n}{11} + 262\frac{1}{2}^\circ\right)$. This formula gives the smoothly flowing curve of Fig. 6, which coincides, as fairly as may be, with the curve of actual variations. For the minimum epoch the formula gives 1878·73, and for the maximum 1884·23,—dates which fall suggestively near those of the corresponding phases of solar disturbance.

Into this interesting speculation it is impossible at present to enter further, beyond remarking, as was said at the beginning, that the increase of the population during the last ten years was probably above the average, and too rapid to be maintained. The hypothesis that it is subject to a variation in the eleven-year period leads to the result that the mean for a long term of years is only 4·576 per thousand, instead of 5·49. Now, in the Report on the Census of 1881, the Census Officer, Mr. Edmund White, calculated that the population, as reported, increased only 2·33 per thousand per annum between 1853 and 1881; but it is pointed out that this result was vitiated by an over-estimation in 1853, when the individual members of the population were not counted by name, but only the total number of each family was entered in the census forms. In the sixteen years from 1865 to 1881 the rate of increase was 4·48, and as these years included a fair proportion of good and bad, the rate of movement is probably near the truth. It differs only by a small fraction from the mean rate given by the above formula, according to which the population might be expected to double itself in 152 years, notwithstanding the already great pressure on the soil. In the same Census Report, from the distribution of the population according to age, a mean death-rate of 39·5 per mille is arrived at. This agrees sufficiently closely with the rate here found to warrant the conclusion that the corrections applied to the numbers actually registered cannot be far wrong.

Fig. 7 shows the variations of the average rainfall of the province, for which the general mean of the ten years


MILLIONS
1878
1880
1882
1884
1886
48
46
FIG. 1.
FIG. 2.
+
·2
0
·2
-
FIG. 3.
50
PER 1000
30
FIG. 4.
FIG. 5.
+
4
0
4
-
FIG. 6.
50
INCHES
30
FIG. 7.
MILLION
1·0
·9
FIG. 8.


J F M A M J J A S O N D
80°
60°
FIG. 9.
30°
FIG. 10.
20°
FIG. 11.
IN.
2
0
34
30
FIG. 12.
24
20
FIG. 13.
1
0
FIG. 14.
FIG. 15.
0·4
0·3
FIG. 16.
44
40
FIG. 17.

was almost exactly 38 inches. Neither the birth-rate nor the rate of increase has any distinct relation to the rainfall, but there are very evident indications of such a relation as regards the rate of mortality. The first seven years witnessed great fluctuations in the rainfall, and these were almost exactly parallel in the death-rates, the wettest years being those of greatest mortality. During the last three years, on the other hand, the death-rate increased slightly as rainfall diminished, and *vice versâ*. Amongst the principal causes of death, cholera and small-pox vary enormously in their prevalence from year to year, these diseases being of an epidemic nature; but the variations do not seem to be related in any way to the rainfall. On the contrary, those from endemic malarial fevers, represented by Fig. 8, follow the rainfall variations very closely, and they are the chief factors in the general mortality.

In my former article, the death-rates from various causes were compared with the prevailing meteorological conditions, not year by year, but month by month. If a similar method be adopted with the statistics of the ten years now available, the conclusions arrived at in the former paper are fully borne out as regards all their more important points, only needing, in one or two instances, slight modifications in detail.

The next table gives the total mortality for each month, computed as a rate per mille per annum, and also the rates for certain specified causes of death, which can in most cases be recognized by the recording agency. In computing these, the registered numbers have all been increased by an allowance for omissions similar to that given above.

Month.	Total Mortality.	Fevers.	Cholera.	Small-pox.	Suicide.	Wounds or Accidents.
January ...	32·71	25·64	0·08	1·16	0·032	0·236
February ...	30·85	22·95	0·08	1·65	0·036	0·255
March ...	30·94	22·26	0·31	2·71	0·062	0·276
April ...	38·26	25·46	2·04	4·79	0·087	0·325
May ...	38·40	25·55	2·32	4·75	0·080	0·366
June ...	36·29	24·12	3·11	3·20	0·083	0·508
July ...	31·41	20·88	2·59	1·71	0·078	0·543
August ...	38·67	26·94	3·08	0·65	0·077	0·535
September ...	45·27	34·39	2·19	0·26	0·084	0·532
October ...	58·60	48·77	2·06	0·12	0·076	0·404
November ...	54·67	46·07	0·79	0·18	0·050	0·276
December ...	42·53	36·12	0·36	0·51	0·034	0·228
Year ...	39·90	29·98	1·58	1·81	0·066	0·374

This table shows how utterly insignificant as causes of death are cholera and small-pox, the two most dreaded diseases, by the side of fevers, which account for three-fourths of the total mortality.

The monthly mean values of the three chief climatic factors for the last ten years may be compared with the preceding figures. They are:—

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Year.
	°	°	°	°	°	°	°	°	°	°	°	°	°
Mean temperature ...	59·4	64·5	75·0	85·7	90·7	91·2	85·0	83·7	83·2	77·8	67·9	60·3 ...	77·0
Range of temperature ...	27·5	27·4	30·0	30·7	27·1	21·3	13·9	13·1	16·0	25·3	30·7	28·9 ...	24·3
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Rainfall	0·87	0·53	0·56	0·22	0·84	4·60	11·91	10·63	6·07	1·38	0·06	0·42 ...	38·09

These are computed on the average of the whole province, exclusive of the higher hill stations.

The relations of the rates of mortality to these climatic causes will be best seen from the curves marked Fig. 9 to Fig. 16. The general death-rate, and that of fever mortality, which follows it closely, have two maxima in October and April or May, and two minima in July and February or March. The secondary maximum in May is a very important feature in the fever curve (Fig. 13), but rises noticeably above the preceding minimum in Fig. 12, which represents the general mortality, owing to the influence of small-pox and, to a small extent, of cholera. The fatal prevalence of fever seems to be altogether uninfluenced by the temperature (represented by Fig. 9), and its variations are almost exactly opposed in phase to those of the rainfall, shown in Fig. 11; but the maxima and minima nearly coincide in time with those of the daily range of temperature (Fig. 10). These statistics therefore confirm the general experience that people are most subject to fever when the nights are chilly and the days hot. If we neglect the secondary maximum in the hot season, Figs. 11 and 13, representing rainfall and fever mortality respectively, will be observed to be almost identical in form, except that the latter is displaced three spaces to the right. This means that malarial fevers are directly dependent on rainfall, in their annual variation as in the variations from year to year; but it takes about three months in Northern India for the malarial conditions brought about by the rainfall (which probably depend on the growth and decay of vegetation) to attain full development. Dr. Meldrum has shown, in several of his annual reports, that in Mauritius the highest fever mortality follows the maximum rainfall at an interval of about two months, and in Northern India a parallel rule seems to hold, except that the interval is slightly longer.

The meteorological conditions predisposing to cholera are evidently heat and moisture, the disease being more prevalent than usual during the whole hot season from April to October, and dying out in the winter. The cholera curve (Fig. 14), which is drawn on a scale four times more open than that for the total mortality, has two maxima in June and August, and a secondary minimum in July. In most years the first maximum falls in April or May, but it has been thrown forward to June in the average for the last ten years by the excessive mortality of last June, when over 50,000 deaths from cholera were registered.

Regarding small-pox (Fig. 15), shown on the same enlarged scale as cholera, all the remarks in my former article hold good. This disease is at a maximum in April and May, and it diminishes rapidly during the rains, until it almost dies out in October and November. The conditions most favourable to its spread seem to be a high wind, and very dry, or perhaps rather very dusty, air; and the number of fatal cases may be almost exactly represented as a direct function of the wind velocity and the dryness of the air. This result is completely in accordance with all that is known about the cause and mode of propagation of the disease.

Fig. 16 gives the annual variation of the deaths by violence, including under this head both suicides and wounds. The curve is a very smoothly flowing one, with a distinct annual minimum at the coldest time of the year, a steady rise through the dry hot season, and relatively high ordinates throughout the rainy season. The scale of the curve is ten times more enlarged than that of the cholera curve, or forty times larger than the scale of total mortality or fever; but though this magnification renders the annual variation visible, it does not reveal any irregularity except a slight increase in September, when, owing to a long "break," or the premature cessation of the rains, the weather sometimes becomes very much hotter than in July or August. This September maximum is more distinct in the suicide ratios than in those of deaths from wounds. It appears, therefore, that these fatalities from crime, instead of disease,

are distinctly subject to climatic causes, and the explanation given in my former article, which attributes them to irritability of temper consequent on long-continued heat and moisture, is the best I can put forward.

When the birth statistics are analyzed with reference to the annual period, results equally striking and curious are brought out. The numbers registered, when tabulated month by month, corrected for the causes of error mentioned at the commencement of this article, and thrown into the form of average rates per thousand per annum, give the following table, in which also the monthly ratios, which are for nine years only, have been slightly altered so as to make the annual mean equal to that already found for ten years:—

Month.	Males.	Females.	Total.	Number of Males to 100 Females.
January	22·67	21·92	44·59	103·42
February	22·31	21·53	43·84	103·67
March	20·72	19·95	40·67	103·86
April	20·17	19·30	39·47	104·51
May	18·46	17·64	36·11	104·65
June	18·12	17·30	35·43	104·74
July	20·80	19·70	40·50	105·59
August	25·81	24·72	50·53	104·41
September	28·85	27·86	56·71	103·55
October	28·30	27·41	55·71	103·25
November	25·89	25·15	51·04	102·94
December	25·36	24·88	50·24	101·52
Year	23·12	22·28	45·40	103·77

From the existence of the *Holi* festival among the Hindus, and of similar spring festivals, accompanied with lascivious songs and dances, among many barbarous tribes, as well as from the traces of such festivals still surviving in Europe, and the hints given by classical writers regarding the nature of certain annual religious mysteries performed by the early Greeks and Romans, anthropologists have thought that possibly, during prehistoric times, the human species, like the lower animals in a state of nature, had an annual pairing-time. If any traces of such a condition still survive, we may with some confidence look for them in India, where a large number of the poorer classes are chronically on the verge of starvation, and the different seasons are sufficiently marked in character to affect people differently both in body and in mind. The birth-rates in the above table, represented by Fig. 17 in the diagram, exhibit a most distinct annual variation, smoother and more uniform in character than any of the mortality curves, and with a range equal to nearly 50 per cent. of the mean value. The minimum falls in June and the maximum in September,—dates which point to a maximum of conceptions in December, and a minimum in September. The latter month is near the end of the long and depressing hot season, when malarial influences are rapidly increasing to a maximum, the food-supply of the year is nearly exhausted, and there is the greatest tendency to suicide. The births, as well as the deaths, therefore, show that at the end of the rains the vitality and energy of the people have reached low-water mark.

In December, on the other hand, not only is the salubrity of the country greatly increased, as shown by the rapid diminution of nearly every cause of death, but food is again cheap and abundant. The crops of millet, on which the poorer classes live, are sown in July and reaped in November. During December and the latter half of November they are threshed out, and then is the season for paying the village functionaries and labourers their share of the produce. Consequently food is more abundant at this time of the year than at any other, and as a result of these conditions we find a large number of births the following September and October.

It thus appears that among the poorest of the population there is probably still a more or less distinct annual reproductive season, but instead of being determined by the returning warmth of spring, as must have been the case in prehistoric Europe, it follows the annual return of healthy conditions with abundant food-supply. That the *Holi* festival occurs in spring, instead of in December, is perhaps to be accounted for as a survival from a time when the ancestors of the Hindus lived in a colder climate.

In the last column of the table are given the monthly values of the ratio of males to females at birth. This appears to be subject to a small but distinct annual variation, with a maximum in July, and a minimum in December; but whether this is a remote and obscure physiological effect of the annual march of the seasons, or only a chance arithmetical result, I cannot say.

Allahabad, February 8. S. A. HILL.

ON THE ORBITS OF AEROLITES.[1]

MY studies have led me to the following three propositions:

1. The meteorites which we have in our cabinets and which were seen to fall were originally (as a class, and with a very small number of exceptions), moving about the sun in orbits that had inclinations less than 90°; that is, their motions were direct, not retrograde.

2. The reason why we have only this class of stones in our collections is not one wholly or even mainly dependent on the habits of men; nor on the times when men are out of doors; nor on the places where men live; nor on any other principle of selection acting at or after the arrival of the stones at the ground. Either the stones which are moving in the solar system across the earth's orbit move in general in direct orbits; or else for some reason the stones which move in retrograde orbits do not in general come through the air to the ground in solid form.

3. The perihelion distances of nearly all the orbits in which these stones moved were not less than 0·5 nor more than 1·0, the earth's radius vector being unity.

The first and thirds propositions are limited strictly by their terms to the meteorites from stone-falls actually witnessed, and also represented by specimens in some one or more of existing collections. The investigations that have led to them have been limited to the same stone-falls. This is not because any line of separation is suspected to exist astronomically between the stone-furnishing and detonating meteors, or even between them and the shooting stars, but because, for manifest reasons, any facts established about these stones have a greater value than similar facts about meteors from which no stones have been secured.

About 265 observed falls are represented by specimens in existing collections. The history of these falls I have searched out with no little pains, so far as the material for such history could be found in books accessible to me. Every direct statement and every indirect indication which I have obtained about the paths of these meteors through the air have been carefully considered, and their meaning and value duly estimated. The determination of the path of a stone-furnishing meteor through the air is greatly aided by the fact that we know at once one point of the trajectory, viz.: the point where the stone strikes the ground. To this fact may usually be added another, viz.: that some of the observations are by persons near the place of fall, and hence their statements of direction, so far as we may trust them, have peculiar significance. In individual cases it will be found that not much reliance can be placed upon the asserted direction of the meteor's motion. But when the results are all

[1] "Upon the relation which the former Orbits of those Meteorites that are in our collections, and that were seen to fall, had to the Earth's Orbit" by H. A. Newton. (From the *American Journal of Science*, July 1888.)

collated there is such a general agreement in support of the first and third propositions set forth above that I am very confident that they are true.

The orbit of a meteoroid about the sun is wholly given when we know these three things, the time when it enters the air, the direction of its motion, and the velocity. The velocity cannot be easily measured directly. But the connection between meteors and comets will be assumed as fully proven. The velocity of the meteoroids (neglecting the increase due to the earth's attraction), ought then to be that of the comets, at the same distance from the sun. The greatest cometary velocity at the distance unity is $\sqrt{2}$, the earth's velocity being unity. The smallest velocity for any known comet is that of Encke's comet, which at the earth's mean distance from the sun is 1·244. It seems safe, therefore, to assume that the meteorites we are considering had velocities relative to the sun not greater than 1·414, nor less than 1·244.

The direction of a meteor's motion through the air is to be determined solely by the evidence of observers of the stone-fall. This evidence needs to be carefully collated, especially when statements apparently conflict. A judicial temper of mind must be preserved in estimating the meaning of the statements, lest the evidence be twisted to the support of some preconceived notion. Knowing the danger, I have tried to keep my own mind free from bias.

We need not know the *exact* day, but we must know the time of day of the stone-fall, else the direction through the air cannot be used. This throws out about one-fifth of the total number of falls named above,—there being no statement of the time of day of the fall attainable. There are left 210 different cases available for use. For 94 of these there is no reliable statement of the direction of the motion of the meteor. We know only the day and the hour. Even this, however, is of some value, since we know that the meteor must have been moving downward at the place of fall; that is, from some point of the heavens then above its horizon. For 116 stone-falls the direction of the motion of the meteor is more or less definitely indicated by the statements of observers, or by the statements of those who have inquired into and reported the facts of the falls.

We may then divide the observed stone falls into three groups which will be separately considered: (*a*), 116 falls for which we have statements as to the direction of the path through the air; (*b*), 94 falls of which we know the time of day; (*c*), 50 or more falls of which the history is too scanty to give the time of day.

There is frequent occasion to speak of two points on the celestial sphere for which the English language has no good names. These are the point from which a body is moving, and the point to which a body is moving. These two points are opposed to each other, as north is to south, east to west, zenith to nadir. The words *quit* and *goal* will be used to denote these two points. The *earth's quit* is that point of the ecliptic from which the earth is moving, the *earth's goal* that point to which the earth is moving; the one being about 90° ahead of the sun in the ecliptic, the other 90° behind it. A *meteor's quit* is that point of the heavens from which the meteor is moving; its *goal* that point of the heavens to which it is moving. The motion may be that relative to the earth, in which case the point of the celestial sphere from which it is moving is the meteor's *relative quit*. Thus the relative quit of a meteor when it is entering the air must be above the horizon of the place of entrance, inasmuch as the meteor must be moving downward. If a meteoroid's motion be corrected for the earth's motion the direction of its absolute motion about the sun is obtained, and then the two points of the celestial sphere from which and to which the meteoroid is moving are its *absolute quit* and its *absolute goal*.

The observations have been treated graphically. They do not demand nor do they admit of greater accuracy in methods of discussion than can be used in graphic processes, and these processes have many advantages over numerical computations. A stereographic projection of two hemispheres was prepared and printed, upon which there were three sets of coordinate lines from three sets of poles. The three sets of points were the angles of triquadrantal triangles. Thus the lines were drawn to represent at intervals of 10° the distances and directions from the poles P, P, S, E, and G, Q, (Fig. 1). In the engraved figure these coordinate limes are omitted. The common diameter of the two hemispheres E S E was made to represent the ecliptic, and the sun was placed at the centre or at the edge of one of the hemispheres. The points P would then be the poles of the ecliptic, and if S be the place of the sun the earth's quit will be Q, and the earth's goal G.

To treat any single meteor a large celestial globe was first set for the time and place of the fall. Upon the globe the celestial latitude and longitude of the zenith and of the west-point were then measured. The day of the year gave the sun's longitude. The zenith and west-point could then be marked upon the chart, after which it was easy to draw the circles representing the meridian and the prime vertical. The stereo-graphic projection was peculiarly advantageous in this work as all circles are represented by circles, and angles are conserved in the projection. The effort was then made to mark upon the chart the meteor's relative quit as accurately as the observations permit, or rather to describe an area within which the quit was probably or certainly located

Some of the 116 meteorite quits have been heretofore fairly well determined by other persons, or they can be so determined. This is the case with the meteors of Agram, Weston, Orgeuil, Pultusk, Iowa, Rochester, Estherville, Krahenberg, Khairpur, Vendome, etc. For other cases we are able by comparing the various statements of observers to locate approximately the relative quit. But for a considerable number of the falls we have to be content with the simple statement that the stones came from the north, or from the northeast, or from the south-southeast, or from some other similarly defined direction. When this has been the case I have taken a point 20° above the horizon in the direction indicated, and considering this as the centre of an area of considerable size within which the quit was probably located, have treated the point itself as the meteor's quit.

These observations of direction in some cases will be in error, or will be perverted in reporting, as every one who has tried to reconcile numerous accounts of a meteor has unpleasantly learned. But when the statements have come from persons who saw the stones come down, they are usually of much more value than similar reports about ordinary meteors. In any case when the reports are single they must be taken for what they are worth. I have plotted them as given.

In several notable instances where there are full accounts I have not been able to accept the conclusions heretofore arrived at as to the direction of the meteor's path. Thus, Dr. Bowditch made the path of the Weston meteor to be from north to south and parallel to the horizon. I make it to have moved from a point N. 40° W., 35° high. The Cold-Bokkeveld meteor was described by Sir Thomas Maclear as moving from the west-north-west. It apparently moved in the opposite direction; that is, from the east-south-east. The l'Aigle meteor was described by M. Biot as moving from the south-south-east, whereas it is well nigh certain that it came from the north-west. In like manner the Stannern meteorite was assumed by von Schreibers to have come from the north-north-west, whereas there are reasons of great weight for believing that it came from the opposite direction. I may add that these and other like changes are not made under any pressure or bias to prove my proposi-

tions. In fact three of the four changes just named make the evidence for my conclusions weaker instead of stronger.

In the treatment of the observations several quantities have been neglected as not large enough to be comparable with the probable errors of the observations themselves. Thus the effect of the earth's attraction in changing the direction of motion, or what has been called the *zenithal attraction* of the quit, has been allowed for only in a general way. So the earth's quit and goal are treated as being exactly 90° from the sun; or, in other words, the earth's orbit has been treated as a circle. In like manner the motion of the place of fall due to the earth's rotation on its axis has not been taken account of.

Having located upon the chart the meteor's relative quit we have next to construct its absolute quit. This evidently lies on the great circle joining the relative quit to Q (Fig. 1), which, when the sun is at S is represented on the chart by a straight line through Q, together with its corresponding line through G. When the absolute velocity of the meteroid in its motion about the sun is given, the place on this circle of the absolute quit can be determined by combining by the parallelogram of velocities the motions of the earth and of the meteoroid. The following table is an abstract of a larger one used in this reduction, and is constructed for the limiting velocities 1·414 and 1·244:—

Table showing the Distances from the Earth's Quit to the Absolute Quit of a Meteoroid for Different Distances from the Earth's Quit to the Relative Quit of the Meteoroid.

Distance from Q to relative quit.	Distance from Q to absolute quit. $v = 1·414$.	$v = 1·244$
30°	9°·3	6°·3
60	22·1	15·8
90	45·0	36·5
120	82·1	75·8
150	129·3	126·3
180	180·0	180·0

In the following constructions the maximum velocity of the meteoroid has been used. When the meteoroid's relative quit is known as a point the absolute quit is at once constructed. If, however, we have an area within which the relative quit is probably located we may mark off with equal facility points on the boundaries of the area within which the absolute quit is probably located. If the former area is a circle the latter will be an oval. The centre of the circle does not correspond exactly to the centre of the oval, but by applying a correction to the table the centre of the oval absolute quit area can be directly constructed from the centre of the circular relative-quit area.

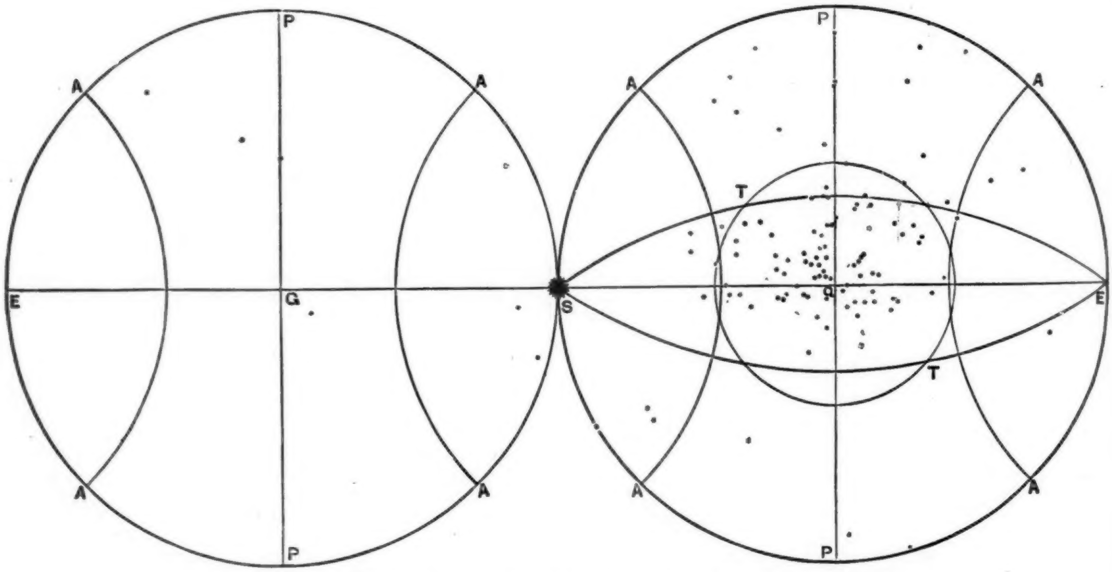


FIG. 1.—Showing the distribution of 116 meteorite quits relatively to the sun's place and to the earth's quit.

In Fig. 1 I have given in a single diagram constructed on a stereographic projection, the results for 116 stone-falls. The best determinations which the accounts admit of for the meteor's direction were first made out. Then the centre of the probable quit area in each case was assumed to be the actual quit. When only the quarter of the heavens from which the stones came is stated the centre of probable area was taken 20° above the horizon. Interpreted thus, the stars in Fig. 1 represent the places of the 116 absolute quits relatively to the place of the sun, S, and to that of the earth's quit and goal, Q and G.

Let us denote any one of these quits (or stars), by the letter q. The elements of the orbit in which the corresponding stone was formerly moving can be easily obtained from the projection. The earth's longitude on the day of fall is the longitude of the node. The angle qSQ is the inclination of the orbit to the ecliptic, and its amount is at once read off on the projection. The orbit has been assumed to have been a parabola. Hence, twice the complement of qS was the angular distance of the stone from its perihelion. If $q\mathrm{S} > 90°$, the perihelion had not been reached; if $q\mathrm{S} < 90$, the perihelion, had been passed. The perihelion distance was $\sin^2$QS. If, however, it be assumed that the orbit was a long ellipse of given major axis, the place of the absolute quit, q, moves somewhat nearer to Q along the line qQ, the angle in the plane of the orbit from perihelion was a little more than twice the complement of qS, and the perihelion distance somewhat less than $\sin^2 q$S. But all these

quantities are easily computed in terms of the assumed major axis. With a semi-major axis as large as 5 the change in Fig. 1 would not be so considerable as to modify any conclusions we can deduce from the grouping of the stars.

The most noticeable fact revealed by the figure is the clustering of the stars about the point Q. All but seven of the 116 meteor quits are in the Q hemisphere; that is, had orbits whose inclinations were less than 90°. One hundred and nine followed the earth, seven met it. Again the two lines STE are drawn to represent circles inclined 35° to the ecliptic. More than two-thirds of the meteor quits lie between these two lines; hence, over two-thirds of the orbits were inclined less than 35° to the ecliptic, the motion being direct.

It should be said that this clustering of the points near Q is somewhat exaggerated in the figure by the nature of the stereographic projection. The scale of distances near Q differs from that near the circumference. But this does not affect the distribution between the hemispheres.

It has been assumed that certain centres of quit areas were themselves the quits. Can the condensation of the quits near Q have been caused in any way by this assumption? Or, is it possible that general errors of observation, or inaccuracy of reporting, could have been the cause? To answer this question let us suppose that there had existed a law that led to condensation of the relative quits in any manner whatever. The effect of the errors of observing or reporting, and also the effect of the assumption above stated, would be toward scattering these relative quits over the heavens more equably, and thus masking the law. Then when the relative quits thus unduly scattered are reduced to absolute quits there might be as a result a tendency towards condensation near Q. If, however, we draw the circle TT, enclosing those absolute quits whose relative quits are in the hemisphere next Q, the general tendency of the errors in question would be towards equalizing the number of absolute quits within to those without the circle TT. Now, the number of stars is nearly twice as great within

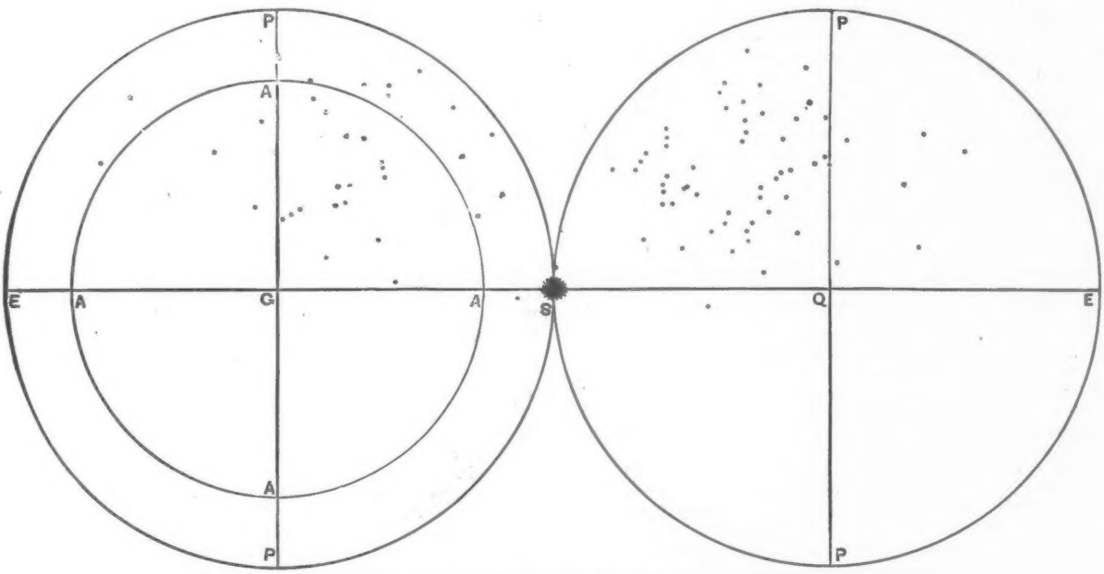


FIG. 2.—Showing relatively to the sun's place, the zeniths for the time and place of 94 stone-falls.

as without the circle. The condensation about Q, shown in Fig. 1, exists therefore in spite of, and not in consequence of, these errors. With a good deal of confidence do I conclude that these 116 meteors were, as a class and with probably a very few exceptions, before coming into the air following the earth in its orbit about the sun.

Another fact of great interest is also shown by the grouping of the points in Fig. 1. In general these stones did not go in their orbits very near to the sun. Assuming that the orbits were parabolas we have for all the stones whose perihelion distances were less than one-half, $\sin^2 q S < \frac{1}{2}$. If there be drawn circles, AA, AA, 45° from S and from E, then will all the stones whose absolute quits were in the central zone, A P P A A A which is bounded by the circles A A, have perihelion distances greater than one-half and less than unity. Of these there are 103 out of a total 116. If the same orbits are assumed to have had semi-major axes equal to 5, then the circles A A would have to be drawn a fraction of one degree farther from S and from E to serve as the limiting curve to orbits whose perihelion distances exceed one-half.

It appears from Fig. 1 that these 116 stones were, with a few exceptions, following the earth in their orbit about the sun. This could happen from either one or more of three possible causes:

Firstly, that nearly all the stones in the solar system are moving in direct orbits, very few in retrograde orbits;—

Or, secondly, that stones moving in retrograde orbits for some reason, as for example their great relative velocity, may not have been able to pass through the air and to reach the ground in solid form;—

Or, thirdly, that stones moving in such retrograde orbits, and coming through the air, may be falling while men sleep, or for some like reason may fail to be found. In other words, the effective cause may work above the air, in the air, or below the air.

Let us assume, as an hypothesis, that neither of the first two are the true causes. In that case we should have the stones moving in every direction as they cross the

earth's orbit. There should be about as many orbits having retrograde motions as direct motions. Hence the absolute quits of all stones coming into and hence, by hypothesis, coming through the air, should be symmetrically distributed in their longitudes relative to the sun. At least there should be as many absolute quits in the G-hemisphere as in the Q hemisphere (Fig. 1). Take account now of the earth's motion and locate the relative quits. All these stones whose absolute quits lie outside of the circle T T will have their relative quits in the G-hemisphere. Upon the hypothesis of parabolic orbits and of an equable distribution of the absolute quits over the celestial sphere the number of relative quits in the G-hemisphere should be to those in the Q-hemisphere as $1 + \cos\frac{\pi}{4} : 1 - \cos\frac{\pi}{4}$, or as 17:3. The relative quits should then be *very much* more numerous in the G-hemisphere than in the Q-hemisphere.

Furthermore, suppose that the heavens visible at a given time and place, are divided by a vertical circle into two halves; and suppose that this vertical circle is at right angles to the plane containing the zenith and the earth's quit and goal. That half of the visible heavens that lies towards the earth's goal may be called the goal-half, the other half may be called the quit-half of the visible heavens. In any given period there should evidently be, under the several hypotheses stated, many more stones coming into the air and reaching the ground directed from the goal-half than there should be directed from the quit-half of the visible heavens. Still further, since this proposition applies to any epoch whatever, we may apply it to 116 periods covering the times of the 116 stone-falls, that is, to the 116 stone-falls themselves. Many more of these should (under the hypotheses stated) have come from the goal-half than from the quit-half of the visible heavens.

If, then, the relative quit of each of these 116 stones is supposed to be carried around in azimuth 180°, the altitude being unchanged, the 116 distances from each new place of the quit to the earth's quit for the epoch of the fall should, in the average, be decidedly less than the corresponding 116 distances from the actual relative quits to the earth's quit. This should hold true (under the hypotheses stated) no matter what causes below the air may have occasioned the selection of the 116 epochs. The fact that more persons are abroad in the evening hours from 6h. to 10h. p.m., than in the corresponding morning hours, 2h. to 6h. a.m., may well cause that more stones should be secured in the evening than in the morning hours. In the evening hours the earth's quit is above the horizon; in the morning hours the earth's goal. It might easily be that we should for this reason get more stones of direct than of retrograde motions. But the above criterion is entirely independent of any such principle of selection of the epochs. A change of the azimuth of the quits through 180° should cause a larger number of them (under the hypotheses stated) to approach the earth's quit than to recede from it.

I have marked off upon the working sheets the position 180° in azimuth from each of 115 relative quits, the altitude being unchanged, and measured the several distances from the earth's quit. (One fall, Nedagolla, was unavailable). The following is the result. In 44 cases the meteor's quit by the change approaches the earth's quit; in 70 cases it approaches the earth's goal; in one it remains unchanged. That is, instead of a very large majority of the quits moving towards the earth's quit we have nearly two-thirds of them moving the other way. In the reversed position, moreover, we should have had 38 absolute quits in the G-hemisphere instead of 7. These numbers show very decidedly that the hypotheses made above are not true. The principle of selection is not *entirely* below the air, and the numbers testify so markedly against that hypothesis that I feel warranted in adding that the cause is *mainly* either above the air, or in the air.

Between the first and second causes named the materials used for the present discussion do not furnish a *positive* critical test. But if, as I believe, the Stannern stone came from the south, we have at least one instance of stones coming into the air with a velocity of nearly, or quite, 45 miles a second and reaching the ground in solid form. About twenty-five of the quits in Fig. 1 imply velocities of not less than 25 miles a second on entering the air. Large velocities do not seem to be entirely fatal to the integrity of the meteorites. I believe that the first cause was the dominant one rather than the second, yet for a crucial test of the two causes, if one can be found, we must look to a class of facts other than those we have been considering.

We are now in position to consider the other ninety-four stone-falls. In Fig. 2, the construction of which is similar to that of Fig. 1, the stars mark the zenith points for each time and place of the ninety-four falls. A grouping is at once noticeable. They are nearly all in the northern hemisphere, since the observing peoples live there. Those stars in the hemisphere of which S is the pole, that is between the two lines P P and P P, are evidently daylight stone falls, since S is above the horizon for each case. These constitute about seven-eighths of the whole number. The reason for this predominance is manifest. In the night men see the fireball or the train, whereas in the day the first intimation of the stone-fall is usually the hearing of the detonation two or three minutes after the fireball has disappeared. Hence, daylight stone-falls are those whose directions are less likely to be observed, and these ninety-four falls are the ones of which the directions are unknown.

It will also be seen that there are nearly twice as many in the Q-hemisphere as in the G-hemisphere; that is, there are nearly twice as many that fell when the earth's quit was above the horizon as there were when the earth's goal was above the horizon. In general, the former were afternoon stone-falls, the latter forenoon stone-falls. Now the habits of the urban population have not much to do with these daylight meteors, for the fireballs were not seen. The accounts come from the country, where the stones in general have fallen, and about as many people are there abroad in the forenoon as in the afternoon. If stones came to the ground as often from retrograde as from direct orbits we ought apparently to have had very many more zeniths in the G-hemisphere than in the Q-hemisphere. The contrary being the fact of experience we may reasonably say that the ninety-four stone-falls, about which we know comparatively little, seem decidedly to follow the same laws as the 116 falls about which we know so much more.

This conclusion is greatly strengthened if we take account of the effect of the earth's attraction in carrying the meteor's quit toward the zenith. Any stone must be moving downward when it enters the air. But the earth's attraction must change the direction of its motion during the approach to the earth. Hence the region of the heavens from which a stone can approach the earth is not bounded by the actual horizon, but by a curve which may be treated as a depressed horizon. This depression of the horizon is far greater toward the quit than toward the the goal side of the horizon. The maximum depression for a stone moving in a parabolic orbit is about 17°. It hence follows that when the zenith is more than 73° and less than 90° from G, both the points G and Q are above the depressed horizon, and therefore that the 14 falls whose zeniths are between these limits, that is, are between the circles A A and P E P S, Fig. 2, should be left out of the count. The corresponding region on the Q-hemisphere is less than one degree in breadth, and contains one zenith point. We have left only 20 falls when

the earth's goal alone was above the depressed horizon to be compared with 59 falls when the earth's quit alone was above the depressed horizon.

Of the 50 observed falls constituting the third group, of which the hour of fall is not stated, very few particulars other than the fact of fall are known. Although we are left without the power of saying that they indicate the same law as the other 210 falls, we find at the same time no reason to suspect the contrary. It is not unreasonable to assume that the well-observed stone-falls are good representatives of the whole group, and to affirm the three propositions with which I set out as true, in general, not only for the 210 stone-falls of the first two groups, but for the whole 260 stone-falls which are represented by stones in our cabinets, and in which the stones were seen or known to fall.

It also seems a natural and proper corollary to these propositions (unless it shall appear that stones meeting the earth are destroyed in the air), that the larger meteorites moving in our solar system are allied much more closely with the group of comets of short period than with the comets whose orbits are nearly parabolic. All the known comets of shorter periods than 33 years move about the sun in direct orbits that have moderate inclinations to the Ecliptic. On the contrary, of the nearly parabolic cometic orbits that are known only a small proportion of the whole number have small inclinations with direct motion.

It also follows that in future reductions of these stone-fall observations it will be better to assume that the velocity of the stone in its orbit was not that velocity which corresponds to a parabolic orbit, but that which corresponds to the mean orbit of the comets of short period. The largeness of the perihelion distances has an evident bearing also upon the idea that these stones form the fuel of the sun.

The presentation of the argument here made has been incomplete in that the details of the investigation of individual stone-falls have been entirely omitted. Some of the determinations of the paths are, I think, as complete as I can hope to make them. But others must be regarded as provisional, since I hope to secure respecting them additional data. I hope at some future time to give a complete discussion of all these observed stone-falls. In the past I have been greatly indebted to friends for aid in collecting accounts of the falls, and I heartily thank them therefore. I shall be very grateful also in the future for unpublished observations of the stone-falls, as well as for observations that have been so published as not to be likely to have attracted attention. I bespeak the kindly aid of any who have made or have collected such observations.

NOTES.

AT the time of the Paris Exhibition in 1889, several scientific congresses will assemble in the French capital—congresses of zoology, anthropology, physiology, electricity, dermatology, hygiene. The *Revue Scientifique* expresses a hope that the great congress of electricity in 1881 may be taken as a model for all these assemblies; that attempts will be made, as far as possible, to establish uniformity in scientific nomenclature; and that men of science in other countries will not allow themselves to be deterred by international jealousies from being adequately represented at meetings whose proceedings will relate to matters of universal interest.

AT the next meeting of the British Association there will be a discussion in Section D on the vexed question of the formation of coral reefs. The discussion will be opened by Dr. Sydney J. Hickson.

ON Tuesday evening Mr. W. H. Smith, speaking of the measures with which it would be impossible to deal during the present Session, announced that the Government had decided to drop the Technical Instruction Bill. He deeply regretted that this was necessary, "but perhaps," he added, "there may not be much loss of time, as the Royal Commission on Elementary Education will report shortly on the whole question, and it will be interesting and convenient to the House to have that report before it before attempting to legislate on the subject."

A CONFERENCE of the Executive Committee of the National Association for the Promotion of Technical Education and representatives of branches and co-operating associations was held last Saturday afternoon at the Society of Arts. Afterwards the first annual meeting of the Association was held. Lord Hartington presided, and delivered an able and interesting speech, showing how the establishment of a proper system of technical instruction has been rendered absolutely necessary by the conditions of modern industrial development.

THE anniversary meeting of the Sanitary Institute of Great Britain will be held to-day at 3 p.m. The chair will be taken by Mr. Edwin Chadwick, C.B., who will present the medals and certificates awarded to the exhibitors at the exhibition held at Bolton. Dr. B. W. Richardson, F.R.S., will deliver an address, entitled, "The Storage of Life as a Sanitary Study."

ON Thursday, the 5th inst., Prof. Stokes distributed the prizes to the students at the Medical School, St. Thomas's Hospital. In addressing the students he said that he need not remind them that diligence was the great road to success, and urged that it was a duty to work for our fellow-creatures as well as ourselves. He thought that the two noblest professions were those, one of which assisted in the rectification of man's character and the other in alleviating the results of disease. In the exercise of the medical profession our best feelings were, he thought, called forth. The best foundation was a general liberal education, and although those branches of science which bear directly on medicine might be separated from their practical application, they were in themselves most interesting, and, when studied for their own sakes, were excellent mental training. He was glad to hear from Dr. Ord that St. Thomas's students were successful in athletics, as the cups exhibited testified. In the necessarily sedentary life of a medical student exercise and relaxation should not be neglected, and students did well to study the use of their muscles in athletic pursuits. Sir John Simon, on behalf of the Governors of the Hospital, thanked Prof. Stokes for distributing the prizes, and referred to the high position attained by Prof. Stokes, who, as President of the Royal Society, and representative in Parliament of the University of Sir Isaac Newton, might be said to have gained the best possible prize, but hinted that the happiness of life consisted in its endeavours rather than in its prizes. He concluded by alluding to the retirement of Dr. Ord, whose services as Dean of the Medical School during the past twelve years had been, he felt sure, much appreciated by the Governors of the Hospital, by the medical and surgical staff, and by the students.

THE French Minister of Public Instruction has authorized the following scientific missions:—M. Georges Martin is entrusted with a mission to Sweden and Norway, to study the different educational questions; M. Henry Meyners d'Estrey is sent to explore the mountainous districts of Scandinavia, and to study certain questions connected with ethnography and anthropology; M. Gaston Angelvy, civil engineer, goes to explore the tract of country between Lake Nyassa and the coast of the Indian Ocean, and to visit more particularly the basin of the river Royaurva.

THE Musée Guimet in Paris, which contains specimens of a great number of objects used in religious ceremonies, was

nominally opened some days ago. It will not, however, be opened to the public for several months.

THE meeting which will shortly be held in Paris for the study of tuberculosis, under Prof. Chauveau's presidency, promises to be very interesting and successful.

THE International Congress of "Americanists" will hold its seventh session in Berlin from October 2 to 5 next. The organizing committee has just issued the programme. The first day will be devoted to questions relating to the discovery of the New World, to the history of America before the time of Columbus, and to American geology; the second to archæology; the third to anthropology and ethnography; the fourth to philology and palæography.

IT is proposed that an exhibition, to be called the "Three Americas Permanant Exhibition," shall be established at Washington in 1892 as a memorial of the discovery of America by Columbus. Both Houses of Congress have expressed approval of the scheme. While the subject was being considered by the House Committee on Commerce, Major J. W. Powell, director of the U.S. Geological Survey, pointed out, in an interesting address to the Committee, the benefits that archæologists would be likely to derive from such an exhibition, and the importance of securing without delay the necessary materials.

WE have received the volume containing a report of the Proceedings of the thirty-sixth meeting of the American Association for the Advancement of Science, held at New York in August, 1887. Among the more interesting contents of the volume is the address of Dr. Daniel G. Brinton, vice-President of the Section for anthropology. In this address Dr. Brinton presents a comprehensive review of the data for the study of the prehistoric chronology of America. Speaking of physical characteristics, he says that although the anatomy and physiology of the various American tribes present great diversity they also display a really remarkable fixedness of type. No observer well acquainted with this type could err, he thinks, in taking it for another. "Darwin says that the Fuegians so closely resemble the Botocudos [of Brazil] that they seemed members of the same tribe. I have seen Arawacks from Guiana who in the north-west would have passed for Sioux." According to Prof. J. Kollmann, the results of whose researches on this subject are accepted by Dr. Brinton, the essential physical identity of the American race is as extended in time as in space. Prof. Kollmann has analyzed the cranioscopic formulas of the most ancient American skulls, those from the alleged tertiary deposits of the Pampas, that obtained from Rock Bluff, Illinois, the celebrated Calaveras skull from California, and one from Pontemelo in Buenos Ayres of geologic antiquity. The conclusion at which he arrives is that the earliest Americans—those who were contemporaries of the fossil horse and other long since extinct quadrupeds—possessed the same racial character as the natives of the present day, with similar skulls and a like physiognomy.

ON Monday the atmosphere in the Channel became so rarefied that objects could be seen with extraordinary distinctness at a distance of between 30 and 40 miles from Dover and Folkestone. The *Times* says that the lighthouse at Cape Grisnez, Calais, and the dome of the Cathedral, and Napoleon's Column at Boulogne could be distinctly seen with the naked eye, and every prominent object could be picked out along the French coast. The distance from Dover to Boulogne as the crow flies is 28 miles, and the column is about 2 miles further inland.

THE following telegram from Valparaiso was lately received at Buenos Ayres:—"A rather severe earthquake shock was experienced in Santiago on Sunday, May 13, at 11.30 a.m., and considerable alarm prevailed in consequence of May 13 being the anniversary of the great earthquake in 1647, which laid a large portion of the city in ruins, and which was the origin of the procession of the Señor de Mayo. A severe but short vertical shock occurred here on Tuesday, the 15th, at 8.5 p.m. A strong earthquake shock was felt at Yumbel on the 10th, at 9.15 p.m. A smart earthquake shock, preceded by a long subterranean noise, was experienced in Santiago on Wednesday, the 16th, at 4.55 a.m. The shock was also felt here, but slightly." At Buenos Ayres several earthquake shocks were experienced on the night of Monday, June 4. According to the *Buenos Ayres Standard*, a slight shock was felt at 12.18. Three seconds afterwards a very strong shock occurred, and the oscillation was slow and pronounced. The walls of houses and all movable articles were shaken, and a third shock, which seemed to be nothing more than the subsidence of the second, occurred two seconds afterwards. No serious accident followed the occurrence. Several families, however, were so startled that they rushed out of their houses and sought refuge in the open square. The shocks were felt with more or less intensity all over the province of Buenos Ayres and in Montevideo. As felt in Montevideo the shock passed from south-south-west to north-north-east.

AT the meeting of the French Meteorological Society on June 5, M. Angot communicated a paper on the climate of St. Martin-de-Hinx (Landes) based on observations made since 1864, in which he has determined the diurnal variations of each element. He also announced that as soon as funds were obtained he intended to publish *in extenso* several long series of observations. At several of the places mentioned, including Paris, Marseilles, &c., the observations date from far into the last century. M. L. Teisserenc de Bort communicated a note relative to two earthquakes which occurred at 8 p.m. on the 4th, and at 5 p.m. on May 14 last, in the department of Puy-de-Dôme. M. Moureaux remarked that the magnetograms at Parc-St.-Maur showed no special disturbances at those times. M. Renou paid a tribute to the memory of M. Hervé-Mangon, to whose exertions the separation of the meteorological from the astronomical service was due. This memoir will be printed in the Bulletin of the Society.

A NEW base and its series of salts, belonging to the remarkable group known as "platinum bases," have been obtained by Dr. Heinrich Alexander, of Königsberg. The base itself has the composition $Pt(OH)_2 \cdot 4NH_3O$, and may be considered as the hydroxylamine-platinum compound corresponding to the free base of the well-known green salt of Magnus, $Pt(OH)_2 \cdot 4NH_3$. The chloride of the series was prepared some little time ago by Lossen, but can be most readily obtained, according to Alexander, by mixing a 10 per cent. solution of potassium platinous chloride with hydrochloride of hydroxylamine and an alkaline carbonate. On standing, the deep red liquid becomes decolourized, and the reaction is completed when a yellowish precipitate commences to settle; on the addition of more alkali the new base is immediately and quantitatively precipitated. The precipitate is then dissolved in the calculated quantity of cold dilute hydrochloric acid, and on passing a gentle stream of hydrochloric acid gas through the solution, or on the addition of absolute alcohol, fine colourless needles of the chloride $PtCl_2 \cdot 4NH_3O$ are deposited. These needles are very soluble in water, but, like many other chlorides, are insoluble in concentrated hydrochloric acid. The free base is at once precipitated from this salt on the addition of stronger bases, such as potash and soda, or even ammonia. It is perfectly stable in the air and is extremely insoluble in water and alcohol; it behaves exactly like a true metallic hydroxide, dissolving in acids with formation of the corresponding salts. The sulphate $PtSO_4 \cdot 4NH_3O$, which is best obtained by treating the base with the calculated quantity of sulphuric acid upon a water bath, crystallizes well in short, heavy prisms, difficultly soluble in cold

but better in hot water, the crystals deposited from which contain a molecule of water of crystallization. When heated above 100° C. it violently decomposes with detonation. In a similar manner the phosphate and oxalate of the series were obtained pure and analyzed. The former separates out in microscopic crystals while the latter is deposited in beautiful stellar aggregates of long needles. During the course of the work, two interesting isomeric salts were obtained. When the base is treated with excess of warm hydrochloric acid and the solution allowed to cool, yellow needles of a chloride of the composition $PtCl_2 . 2NH_3O$ fall out. If however potassium platinous chloride be added to dilute solutions of the first chloride, $PtCl_2 . 4NH_3O$, beautiful violet needles of an isomeric salt, $PtCl_2 . 4NH_3O + PtCl_2$, separate out. The two substances are quite distinct, though possessing the same empirical formula, reminding one of the remarkable isomerism so frequently met with among the compounds of carbon.

UNDER the heading of "Psychology" the *American Naturalist* for May has a curious paragraph on "The Monkey as a Scientific Investigator." In the interesting little "Zoo" connected with the National Museum at Washington, there is a fine male grivet monkey (*Cercopithecus erythræa*), who shares a large cage with four opossums. To human beings he shows himself anything but amiable, but "he takes kindly to his strange companions, and they have been the best friends from the first." The attention of the attendant was lately drawn to the cage by the excitement of a crowd in front of it, and on going to ascertain the cause he was surprised to see the monkey seated in the middle of the cage, with one of the opossums lying quietly on her back on his lap, and her head under his arm. "The monkey had just discovered the marsupial pouch of the opossum, and was diligently investigating it. Had he not been a close observer it certainly would have remained unseen, for it was so tightly closed as to be perfectly invisible in its normal condition. The monkey carefully lifted the outer wall of the pouch, and peered into the cavity. Then he reached in with his hand, felt about for a moment, and to the astonishment of everybody took out a tiny young opossum, about 2 inches long, hairless, blind, and very helpless, but alive and kicking. Jock held it up to the light, where he could get a good view of it, scrutinized it with the air of a *savant*, and presently returned it to the pouch, very carefully. After replacing it he looked into the pouch again, and presently drew out another for examination, which he looked at with solemn interest, smelt it, and then carefully put it back. It was thus it became known to the attendants that the old female opossum had the young ones, which had previously been looked for in vain."

SOME time ago an English resident at Canton, Mr. Pitman, bought a curious monstrosity—a sow with six legs. The front part of the body is simple, that is, the animal has one head, one thorax, and two front legs. Behind, all the organs are double. M. Bézaure, the French Consul at Canton, persuaded Mr. Pitman to let him have this strange creature for the Paris Museum of Natural History, where it may now be seen. It is white, with great black spots, and appears to be in perfect health. An account of it, by M. Charles Brongniart, of the Museum of Natural History, appears in the current number of *La Nature*. The separation of the two trunks seems to begin after the dorsal vertebræ; but the animal is so fat that this cannot be precisely determined.

MANY women who are anxious to obtain a University training cannot afford to pay the fees required for residence at one of the colleges or halls in connection with the old Universities. For their benefit Aberdare Hall, Cardiff, was founded; and we are glad to learn that the institution has made steady progress since it was opened in 1885. This year the number of students has doubled. At University College, Cardiff, the students at Aberdare Hall are taught on the same footing as the men students. They generally work for London University degrees, but when they wish to prepare for other examinations the necessary help is gladly given.

THE Irish Exhibition in London has published a useful "Handy-book of Reference for Irishwomen." It is edited by Miss Helen Blackburn, and Mrs. Power Lalor contributes a preface. The volume presents full and accurate information as to women's work in Ireland, and as to the schools and classes in which they may obtain scientific and technical training.

THE annual report of the Geological and Natural History Survey of Canada for 1886 (vol. ii. new series) has been issued. It embodies the results of some of the work of preceding years, and not all of the work of the year for which it is dated. The volume consists of thirteen parts, separately paged and lettered, and relating to various portions of the Dominion from Nova Scotia to British Columbia, and northward to the Arctic Ocean. The parts were issued separately with accompanying maps and illustrations in pamphlet form, as they were received from the printers.

THE new number of the *Mineralogical Magazine* contains, besides abstracts and a full index to vol. vii., the following papers:—On the development of a lamellar structure in quartz-crystals by mechanical means, by Prof. John W. Judd, F.R.S.; on the polysynthetic structure of some porphyritic quartz-crystals in a quartz-felsite, by Major-General C. A. McMahon; on kaolinite, by Alan Dick; note on the occurrence of celestite containing nearly 14 per cent. of free sulphur, by H. J. Johnston-Lavis; notes on hornblende as a rock-forming mineral, by Alfred Harker.

M. VAYNIÈRE has brought out the second of the four parts of his Atlas of invertebrate animals.

IN a circular issued by Mr. Edward S. Holden, director of the Lick Observatory, it is stated that the Observatory buildings will be open to visitors during office hours every day in the year. An hour or so, he points out, can be profitably occupied in viewing the various instruments, and the rest of the stay can be well spent in walks to the various reservoirs, from which magnificent views of the surrounding country can be had. With regard to the admission of visitors at night, Mr. Holden says that, for the present, visitors will be received at the Observatory to look through the great telescope every Saturday night between the hours of 7 and 10, and at these times only. Whenever the work of the Observatory will allow, other telescopes will also be put at the disposition of visitors on Saturdays between the same hours. Mr. Holden hopes that, by setting apart these times for visitors (which allow freer access to the Lick Observatory than is allowed to any other Observatory in the world) all interested may be able to arrange their visits in conformity to them; and that the remaining hours of the week will be kept entirely uninterrupted, in order that the astronomers may do the work upon which the reputation of the Observatory entirely depends.

FROM a report signed by Mr. Edward S. Holden we learn that the trustees of the Lick Observatory, acting on his advice, have provided a photographic attachment to the 36-inch telescope, which will enable this to be used as a gigantic camera for photography. It cannot be used to make maps according to the scheme of the Paris Congress, since that scheme requires a focal length of 13 feet, while that at the Lick Observatory will be 47. But a vast deal of work may be done in connection with applications to astronomy other than the construction of the chart. In the photography of the moon, of the planets, of nebulæ, and comets the Lick telescope will have some important advantages. "But," says Mr. Holden, "it is in the photography of stars—of double and binary stars, of all the fainter stars, of all star

clusters—that the Lick photographic telescope will find its chief application and demonstrate its immense superiority. One of the first works to be done is to photograph the vicinity of all the brighter stars, for the discovery of fainter companions, and for the permanent record of their surroundings. A certain number of stars will be selected and photographed at regular intervals throughout the year. Measures made upon these plates will give the data by which the distances of these stars from the earth can be determined. Similar measures upon photographs of star clusters may serve to give us a clue to the laws which govern the internal structure of these wonderful objects. A continuous series of photographs of the brighter parts of one of the brighter comets will certainly throw a flood of much needed light upon the process of their development."

THE additions to the Zoological Society's Gardens during the past week include a White-thighed Colobus (*Colobus vellerosus* ♂), a Campbell's Monkey (*Cercopithecus campbelli* ♀), a White-Collared Mangabey (*Cercocebus collarii*), a Bosman's Potto (*Perodicticus potto*), a Marabou Stork (*Leptoptilus crumeniferus*), a Black Sternothere (*Sternothærus niger*) from West Africa, presented by Mr. H. H. Johnston, F.Z.S.; two Black-Bellied Sand Grouse (*Pterocles arenarius*) from North Africa, presented by Sir Kirby Green, R.C.M.G.; an Eyed Lizard (*Licerta ocellata*), European, presented by Mr. J. Hopson; a Patas Monkey (*Cercopithecus patas* ♀), two West African Love Birds (*Agapornis pullaria*) from West Africa, a Cormorant (*Phalacrocorax carbo*), British, three Scarlet Ibises (*Eudocimus ruber*) from South America, five Common Chameleons (*Chamæleon vulgaris*) from North Africa, deposited; a Chipping Squirrel (*Tamias striatus*) from North America, five Lesser Pintailed Sand Grouse (*Ptero les exustus* 1 ♂, 3 ♀) from Abyssinia, two Modest Grass Finches (*Amadina modesta*) from Australia, purchased; a Moor Monkey (*Semnopithecus maurus* ♂) from Java, received in exchange; a Spotted Tinamon (*Nothura maculosa*), two Cambayan Turtle Doves (*Turtur senegalensis*), three Chiloe Widgeon (*Mareca chilœnsis*), three Slender Ducks (*Anas gibberifrons*), two Australian Wild Ducks (*Anas superciliosa*), three Mandarin Ducks (*Æx galericulata*), eleven Chilian Pintails (*Dafila spinicanda*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MARKINGS ON MARS.—M. Perrotin, in a more recent communication to the Paris Academy of Sciences, states that the district of *Libya*, the disappearance of which he had recorded a week or two earlier (NATURE, vol. xxxviii. p. 185), has undergone a further change, the "sea" which had so recently covered it having retreated again for the most part, so that the present appearance of the district is intermediate between that which it recently presented and that under which it was seen in 1886. Of the canals M. Perrotin has noticed four, three of which are double, which, starting from the "seas" of the southern hemisphere near the equator, and following a nearly meridional course, extend right up to the north polar ice cap, being traceable across the "seas" which immediately surround the latter. No other observer as yet seems to have traced these canals for such a distance, and across "seas" as well as continents. This observation renders their true character more puzzling than ever, and seems effectually to dispose both of M. Fizeau's just published theory, which explains them by the analogy of the rifts in terrestrial glaciers, Mars being assumed to be in a glacial condition, and of that of Mr. Proctor, who ascribes them to the varying appearances of the Martial rivers when clearly seen or partly veiled by local mists. More detailed observations of these strange markings are needed, and it is to be much desired that as many as possible of actual drawings made at the telescope should be published. It is possible that the comparison of sketches made with different observers and with different apertures, would throw much light on the subject; if, for instance, the appearances were partly optical and due to some effect of diffraction, it would soon become apparent.

COMET 1888*a*, SAWERTHAL.—The remarkable change in brightness which this object displayed about May 20 (NATURE, vol. xxxviii. p. 114) seems to have been well observed, and there is a general agreement that the increase in brightness amounted to 2½ or 3 magnitudes. At Dorpat Herr Blumbach estimated the comet as 9–10 on May 19, and as 7–8 on May 22. Dr. Franz, at Konigsberg, considered the increase as amounting to 3½ magnitudes, estimating the brightness as 5·8 on May 21, whilst Dr. Kammermann, at Geneva, on May 25, reckoned the comet as between the 5th and 6th mags., and the increase as having been between 2 and 3. Father Fenyi, of the Kalocsa Observatory, finds the change of magnitude about the same, but estimates the absolute brightness differently; the recorded magnitudes being: May 20, 9·3; May 21, 7·8; May 22, 6·8; and May 23, 6·8. Father Fenyi also supplies (*Astr. Nach.*, No. 2844) a series of sketches of the comet, showing the changes of shape which have accompanied the changes of brightness, and especially the development about May 28 of a sort of wing on either side of the head. These wings appear, however, to have been seen earlier at other observatories, thus Herr Kortazzi, at Nicolaiew, observed them on May 24, and Herr Wutschichowski gives a beautiful drawing of them under date May 25 (*Astr. Nach.*, No. 2845). The comet does not appear to have been satisfactorily observed with the spectroscope during this period of unusual brilliancy. The outburst was soon over, and the comet speedily returned to its former faintness.

The following ephemeris (*Astr. Nach.*, No. 2838) is in continuation of that given in NATURE, vol. xxxviii. p. 186.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
July 13	1 7 18	50 32·8 N.	0·3352	0·3306	0·029
15	1 7 42	50 55·4			
17	1 7 56	51 17·2	0·3459	0·3331	0·028
19	1 8 2	51 38·4			
21	1 7 57	51 58·8	0·3563	0·3353	0·026
23	1 7 43	52 18·5			
25	1 7 19	52 37·4	0·3664	0·3372	0·025
27	1 6 45	52 55·4			
29	1 6 0	53 12·6	0·3762	0·3389	0·023
31	1 5 6	53 28·9			
Aug. 2	1 4 1	53 44·2 N.	0·3857	0·3405	0·022

The brightness on February 18 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JULY* 15–21.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 15

Sun rises, 4h. 3m.; souths, 12h. 5m. 44·2s.; sets, 20h. 8m.: right asc. on meridian, 7h. 40·8m.; decl. 21° 26′ N. Sidereal Time at Sunset, 15h. 44m.

Moon (at First Quarter July 16, 12h.) rises, 11h. 7m.; souths, 17h. 21m.; sets, 23h. 22m.: right asc. on meridian, 12h. 56·6m.; decl. 0° 35′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury	3 44	11 24	19 4	6 58·6	18 0 N.
Venus	4 4	12 11	20 18	7 45·9	22 9 N.
Mars	12 56	18 2	23 8	13 38·3	11 15 S.
Jupiter	15 38	20 2	0 26*	15 38·0	18 36 S.
Saturn	5 19	13 6	20 53	8 41·8	18 56 N.
Uranus	11 34	17 14	22 54	12 50·3	4 42 S.
Neptune	0 39	8 25	16 11	3 59·5	18 53 N.

* Indicates that the setting is that of the following morning.

Comet Sawerthal.

July.	h.	Right Ascension. h. m.	Declination. ° ′
15	0	1 7·5	50 45 N.
19	0	1 8·0	51 28

Occultations of Stars by the Moon (visible at Greenwich).

July.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
17	ξ′ Libræ	6	19 22	19 40	145 176
18	θ Libræ	4½	21 2	21 23	8 339
19	49 Libræ	5½	0 20	near approach	202 —
19	B.A.C. 5700	6½	22 26	23 14	142 232

July.	h.	
16	13	Mars in conjunction with and 6° 40′ south of the Moon.
18	17	Jupiter in conjunction with and 4° 5′ south of the Moon.
20	0	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	July 15,	21 31	*m*
			,, 20,	21 11	*m*
W Virginis	13 20·3	2 48 S.	,, 16,	22 0	*m*
δ Libræ	14 55·0	8 4 S.	,, 20,	0 44	*m*
U Coronæ	15 13·6	32 3 N.	,, 15,	21 43	*m*
W Herculis	16 31·3	37 34 N.	,, 20,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 19,	2 4	*m*
			,, 19,	22 12	*m*
W Sagittarii	17 57·9	29 35 S.	,, 10,	21 0	*m*
Z Sagittarii	18 14·8	18 55 S.	,, 16,	0 0	*m*
T Serpentis	18 23·4	6 14 N.	,, 20,		*M*
β Lyræ	18 46·0	33 14 N.	,, 15,	1 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 18,		*m*
R Cygni	19 33·8	49 57 N.	,, 19,		*M*
S Aquilæ	20 6·5	15 17 N.	,, 21,		*m*
S Delphini	20 37·9	16 41 N.	,, 18,		*m*
X Cygni	20 39·0	35 11 N.	,, 21,	0 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
The *Perseids*	23	50 N.	Swift, streaks.
Near γ Draconis	269	51 N.	Swift.
,, α Lacertæ	336	49 N.	Swift, short.

GEOGRAPHICAL NOTES.

THE Geographical Society of Paris have decided to avail themselves of the Universal Exhibition at Paris, next year, by convening an International Congress of the Geographical Sciences, to meet in the month of August. There will be two classes of members, subscribing respectively 40 and 20 francs, and each member will be entitled to receive a copy of the publications of the Congress and have a vote in the questions discussed at the meetings. Each Society represented at the Congress will be invited to submit a report on the voyages, explorations, and publications which have most contributed, in the country to which it belongs, to the progress of geography during the past hundred years; the combined reports will afterwards be published with the names of their authors.

DR. H. MEYER has made some important corrections in the preliminary account of his ascent of Kilimanjaro. After verifying and correcting his barometrical observations, he admits that the previously accepted height of 18,700 feet is more accurate than that given by himself, 19,850 feet. He then refers to the dense mist which prevented him from seeing beyond a wall of inaccessible ice, 130 feet high, which his first account indicated as being the terminal point of the peak. It results from these observations that Dr. Meyer did not reach to within 820 feet of the summit of Kilimanjaro, which therefore still remains unconquered.

M. JULES BORELLI, the French traveller, who accompanied M. Rimbaud last year in his interesting journey from Antotto to Harar, is engaged in exploring the country to the south-west of Shoa. The Paris Geographical Society has received some of the results accruing from his journey from Antotto to Jiren, which is situated in 7°42′ N. latitude, and 34°35′ E. longitude. Among these results is the discovery of the sources of the River Hawash, which lie at the foot of Mount Ilfata at the extremity of the Meca range, and not near Mount Dandi, as hitherto supposed. On the summit of the latter peak the traveller found a double lake resembling in shape the figure 8, which is of considerable extent and depth; an affluent of the Gudar, and thus of the Abbay, issues from this lake. He also discovered a deep lake at the bottom of the immense crater mountain known as Mount Harro; the surroundings of this sheet of water are described by the traveller as of incomparable beauty. From this lake, which is named by the natives Wancit, a stream issues and joins the Walga, the source of the latter river being in the summit of Mount Harro. Dr. Traversi, the Italian explorer, made in June, 1887, an excursion into the mountainous region of Urbanagh, lying to the east of the district now being explored by M. Borelli. The chief result of this journey of Dr. Traversi is to throw light on the problem of the hydrographical systems of the Somali and Galla countries. From the summit of Mount Gafat he was able to comfirm his previous observations made near the Suai Lake, with reference to the three lakes above-mentioned and their interconnection.

ON CERTAIN INEQUALITIES RELATING TO PRIME NUMBERS.

I SHALL begin with a method of proving that the number of prime numbers is infinite which is not new, but which it is worth while to recall as an introduction to a similar method, by series, which will subsequently be employed in order to prove that the number of primes of the form $4n + 3$, as also of the form $6n + 5$, is infinite.

It is obvious that the reciprocal of the product

$$\left(1 - \frac{1}{p_1}\right)\left(1 - \frac{1}{p_2}\right)\left(1 - \frac{1}{p_3}\right) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{\mathrm{N}.p}}\right)$$

(where p_i means the ith in the natural succession of primes, and $p_{\mathrm{N}.p}$ means the highest prime number not exceeding N)[1] will be equal to

$$\tfrac{1}{1} + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \tfrac{1}{5} + \tfrac{1}{6} + \cdot \cdot \cdot \cdot + \frac{1}{\mathrm{N}} + \mathrm{R},$$

and therefore greater than log N (R consisting exclusively of positive terms).

Hence

$$\left(1 + \frac{1}{p_1}\right)\left(1 + \frac{1}{p_2}\right) \cdot \cdot \cdot \cdot \left(1 + \frac{1}{p_{\mathrm{N}.p}}\right) > \mathrm{M} \log \mathrm{N},$$

where

$$\mathrm{M} = \left(1 - \frac{1}{p_1^2}\right)\left(1 - \frac{1}{p_2^2}\right) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{\mathrm{N}.p}^2}\right),$$

and is therefore greater than $\frac{2}{\pi}$.

Hence the number of terms in the product must increase indefinitely with N.

By taking the logarithms of both sides we obtain the inequality

$$\mathrm{S}_1 - \tfrac{1}{2}\mathrm{S}_2 + \tfrac{1}{3}\mathrm{S}_3 - \tfrac{1}{4}\mathrm{S}_4 + \cdot \cdot \cdot \cdot > \log \log \mathrm{N},$$

where in general S_i means the sum of inverse ith powers of all the primes not exceeding N; and accordingly is finite, except when $i = 1$, for any value of N. We have therefore

$$\mathrm{S}_1 > \log \log \mathrm{N} + \mathrm{Const.}$$

The actual value of S_1 is observed to differ only by a limited quantity from the second logarithm of N, but I am not aware whether this has ever been strictly proved.

Legendre has found that for large values of N

$$(1 - \tfrac{1}{2})(1 - \tfrac{1}{3}) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{\mathrm{N}.p}}\right) = \frac{1 \cdot 104}{\log \mathrm{N}}.$$

Consequently

$$\left(1 - \frac{1}{p_1}\right)\left(1 - \frac{1}{p_2}\right) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{\mathrm{N}.p}}\right) = \frac{\cdot 552}{\log \mathrm{N}}.$$

This would show that the value of our R bears a finite ratio to log N; calling it θ log N we obtain, according to Legendre's formula,

$$\frac{1}{1 + \theta} = \cdot 552, \text{ which gives } \theta = \cdot 811,$$

so that the nebulous matter, so to say, in the expansion of the reciprocal of the product of the differences between unity and the reciprocals of all the primes not exceeding a given number, stands in the relation of about 4 to 5 to the condensed portion consisting of the reciprocals of the natural numbers.

I will now proceed to establish similar inequalities relating to prime numbers of the respective forms $4n + 3$ and $6n + 5$.

Beginning with the case $4n + 3$, I shall use q_j to signify the jth in the natural succession of primes of the form $4n + 3$, and $q_{\mathrm{N}.q}$ to signify the highest q not exceeding N, N.q itself signifying the number of q's not exceeding N.

[1] N.p itself of course denotes in the above notation the number of primes (p) not exceeding N.

Let us first, without any reference to convergence, consider the product obtained by the usual mode of multiplication of the infinite series

$$S = 1 - \tfrac{1}{3} + \tfrac{1}{5} - \tfrac{1}{7} + \tfrac{1}{9} - \ldots\ \textit{ad inf.}$$

by the product

$$\frac{1}{1-\frac{1}{2}} \cdot \frac{1+\frac{1}{q_1}}{1-\frac{1}{q_1}} \cdot \frac{1+\frac{1}{q_2}}{1-\frac{1}{q_2}} \cdot \frac{1+\frac{1}{q_3}}{1-\frac{1}{q_3}} \ldots\ \textit{ad inf.}$$

It is clear that the effect of the multiplication of S by the numerator of the above product will be to deprive the series S of all its negative terms. Then the effect of dividing by the denominator of the product, with the exception of the factor $1 - \frac{1}{2}$, will be to restore all the obliterated terms, but with the sign + instead of −. Lastly, the effect of multiplying by the reciprocal of $(1 - \frac{1}{2})$ will be to supply the even numbers that were wanting in the denominators of the terms of S, and we shall thus get the indefinite series

$$1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots\ \textit{ad inf.}$$

Call now

$$Q_N = \frac{1}{1-\frac{1}{2}} \cdot \frac{1+\frac{1}{q_1}}{1-\frac{1}{q_1}} \cdot \frac{1+\frac{1}{q_2}}{1-\frac{1}{q_2}} \ldots \frac{1+\frac{1}{q_{N.q}}}{1-\frac{1}{q_{N.q}}}.$$

Q_N, which is finite when N is finite, may be expanded into an infinite aggregate of positive terms, found by multiplying together the series

$$\begin{array}{l} 1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \ldots \\ 1 + \frac{2}{q_1} + \frac{2}{q_1^2} + \frac{2}{q_1^3} + \ldots \\ 1 + \frac{2}{q_2} + \frac{2}{q_2^2} + \frac{2}{q_2^3} + \ldots \\ \ldots\ldots\ldots\ldots\ldots\ldots \\ \ldots\ldots\ldots\ldots\ldots\ldots \\ 1 + \frac{2}{q_{N.q}} + \frac{2}{q_{N.q}^2} + \frac{2}{q_{N.q}^3} + \ldots \end{array}$$

Let

$$S_N = 1 - \tfrac{1}{3} + \tfrac{1}{5} - \tfrac{1}{7} + \tfrac{1}{9} - \tfrac{1}{11} + \ldots \pm \frac{1}{N},$$

then from what has been said it is obvious that we may write

$$Q_N S_N = 1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots + \frac{1}{N} + V - R,$$

where V and R may be constructed according to the following rule: Let the denominator of any term in the aggregate Q_N be called t, and let θ be the smallest odd number which, multiplied by t, makes $t\theta$ greater than N; then if θ is of the form $4n + 1$ it will contribute to V a portion represented by the product of the term by some portion of the series S_N of the form

$$\frac{1}{\theta} - \frac{1}{\theta + 2} + \frac{1}{\theta + 4} - \ldots$$

and if θ is of the form $4n + 3$ it will contribute to − R a portion equal to the term multiplied by a series of the form

$$-\frac{1}{\theta} + \frac{1}{\theta + 2} - \frac{1}{\theta + 4} + \ldots$$

Hence R is made up of the sum of products of portions of the aggregate Q_N multiplied respectively by the series

$$\begin{array}{r} \frac{1}{3} - \frac{1}{5} + \frac{1}{7} - \frac{1}{9} + \frac{1}{11} - \frac{1}{13} + \ldots \\ \frac{1}{7} - \frac{1}{9} + \frac{1}{11} - \frac{1}{13} + \ldots \\ \frac{1}{11} - \frac{1}{13} + \ldots \end{array}$$

of which the greatest is obviously the first, whose value is $1 - S_N$.

Consequently R must be less than the total aggregate Q_N multiplied by $1 - S_N$.

Therefore

$$Q_N S_N + Q_N(1 - S_N) > 1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots + \frac{1}{N} > \log N,$$

i.e. $$Q_N > \log N,$$

from which it follows that when N increases indefinitely the number of factors in Q_N also increases indefinitely, and there must therefore be an infinite number of primes of the form $4n + 3$.

Denoting by M_N the quantity

$$\left(1 - \frac{1}{q_1^2}\right)\left(1 - \frac{1}{q_2^2}\right) \ldots \left(1 - \frac{1}{q_{N.q}^2}\right)$$

we obtain the inequality

$$\left(1 + \frac{1}{q_1}\right)^2\left(1 + \frac{1}{q_2}\right)^2 \ldots \left(1 + \frac{1}{q_{N.q}}\right)^2 > \tfrac{1}{2} M_N \log N,$$

and taking the logarithms of both sides

$$\Sigma_1 - \tfrac{1}{2}\Sigma_2 + \tfrac{1}{3}\Sigma_3 - \ldots > \tfrac{1}{2}\log\log N + \tfrac{1}{2}\log M_N - \tfrac{1}{2}\log 2,$$

where in general Σ_i denotes the sum of the ith powers of the reciprocals of all prime numbers of the form $4n + 3$ not surpassing N.

Hence it follows that $\Sigma_1 > \frac{1}{2}\log\log N$.

If we could determine the ultimate ratio of the sum of those terms of Q_N whose denominators are greater than N to the total aggregate, and should find that μ, the limiting value of this ratio, is not unity, then the method employed to find an inferior limit would enable us also to find a superior limit to Q_N; for we should have $V < \mu Q_N$ added to the sum of portions of what remains of the aggregate when μQ_N is taken from it multiplied respectively by the several series

$$\begin{array}{r} \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \frac{1}{13} - \frac{1}{15} + \ldots\ \textit{ad inf.} \\ \frac{1}{9} - \frac{1}{11} + \frac{1}{13} - \frac{1}{15} + \ldots\ \textit{ad inf.} \\ \frac{1}{13} - \frac{1}{15} + \ldots\ \textit{ad inf.} \end{array}$$

the total value of the sum of which products would evidently be less than

$$(1 - \mu)(S - 1 + \tfrac{1}{3})Q_N.$$

Hence the total value of V would be less than

$$\mu Q_N S + (1 - \mu)Q_N(S - \tfrac{2}{3}),$$

i.e. less than

$$Q_N S - \tfrac{2}{3}(1 - \mu)Q_N,$$

and consequently we should have

$$\tfrac{2}{3}(1 - \mu)Q_N < \log N,$$

i.e. $$Q_N < \frac{3}{2(1 - \mu)} \log N.$$

From which we may draw the important conclusion that if μ is less than 1, *i.e.* if when N is infinite the portion of the aggregate $S_N Q_N$ comprising the terms whose denominators exceed N does not become infinitely greater than the remaining portion, the sum of the reciprocals of all the prime numbers of the form $4n + 3$ not exceeding N would differ by a limited quantity from half the second logarithm of N.

A precisely similar treatment may be applied to prime numbers of the form $6n + 5$. We begin with making

$$S_N = 1 - \tfrac{1}{5} + \tfrac{1}{7} - \tfrac{1}{11} + \tfrac{1}{13} - \tfrac{1}{17} + \tfrac{1}{19} - \ldots$$

We write

$$Q_N = \frac{1}{1-\frac{1}{2}} \cdot \frac{1}{1-\frac{1}{3}} \cdot \frac{1+\frac{1}{r_1}}{1-\frac{1}{r_1}} \cdot \frac{1+\frac{1}{r_2}}{1-\frac{1}{r_2}} \ldots \frac{1+\frac{1}{r_{N.r}}}{1-\frac{1}{r_{N.r}}}$$

We make

$$Q_N S_N = 1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots + \frac{1}{N} + V - R.$$

We prove as before that

$$R < (1 - S)Q_N,$$

and thus obtain

$$Q_N > \log N,$$

and then putting

$$M_N = \left(1 - \frac{1}{r_1^2}\right)\left(1 - \frac{1}{r_2^2}\right) \ldots \left(1 - \frac{1}{r_{N.r}^2}\right),$$

and finally noticing that

$$\frac{1}{1 - \frac{1}{2}} \cdot \frac{1}{1 - \frac{1}{3}} = 3,$$

we obtain

$$\left(1 + \frac{1}{r_1}\right)^2\left(1 + \frac{1}{r_2}\right)^2 \ldots \left(1 + \frac{1}{r_{N.r}}\right)^2 > \tfrac{1}{3} M_N \log N.$$

Taking the logarithms of both sides of the equation, we find

$$\Theta_1 - \tfrac{1}{2}\Theta_2 + \tfrac{1}{3}\Theta_3 - \ . \ . \ . \ . \ . > \tfrac{1}{2}\log\log N + \tfrac{1}{2}\log M_N - \tfrac{1}{2}\log 3,$$

where Θ_i means the sum of ith powers of the reciprocals of all the prime numbers, not exceeding N, of the form $6n + 5$.

Either from this equation or from the one from which it is derived it at once follows that the number of primes of the form $6n + 5$ is greater than any assignable limit.

Parallel to what has been shown in the preceding case, if it could be ascertained that the sum of the terms of the aggregate Q_N whose denominations do not exceed N bears a ratio which becomes indefinitely small to the total aggregate, it would follow by strict demonstration that the sum of the reciprocals of the primes of the form $6n + 5$ inferior to N would always differ by a limited quantity from the half of the second logarithm of N.

It is perhaps worthy of remark that the infinitude of primes of the forms $4n + 3$ and $6n + 5$ may be regarded as a simple rider to Euclid's proof (Book IX., Prop. 20) of the infinitude of the number of primes in general.

The point of this is somewhat blunted in the way in which it is presented in our ordinary text-books on arithmetic and algebra. What Euclid gives is something more than this:[1] his statement is, "There are more prime numbers than any proposed multitude (πλῆθος) of prime numbers"; which he establishes by giving a formula for finding at least one more than any proposed number. He does not say, as our text-book writers do, "if possible let A, B, ... C be *all* the prime numbers," &c., but simply that if A, B, ... C are *any* proposed prime numbers, one or more additional ones may be found by adding unity to their product which will either itself be a prime number, or contain at least one additional prime; which is all that can correctly be said, inasmuch as the augmented product may be the power of a prime.

Thus from one prime number arbitrarily chosen, a progression may be instituted in which one new prime number at least is gained at each step, and so an indefinite number may be found by Euclid's formula: *e.g.* 17 gives birth to 2 and 3; 2, 3, 17 to 103; 2, 3, 17, 103 to 7, 19, 79; and so on.

We may vary Euclid's mode of generation and avoid the transcendental process of decomposing a number into its prime factors by using the more general formula, $a, b, \ldots c + 1$, where $a, b, \ldots c$, are any numbers relatively prime to each other; for this formula will obviously be a prime number or contain one or more distinct factors relatively prime to $a, b, \ldots c$.

The effect of this process will be to generate a continued series of numbers all of which remain prime to each other: if we form the progression

$$a,\ a + 1,\ a^2 + a + 1,\ a(a + 1)(a^2 + a + 1) + 1,\ \ldots$$

and call these successive numbers

$$u_1,\ u_2,\ u_3,\ u_4,\ \ldots$$

we shall obviously have

$$u_{x+1} = u_x^2 - u_x + 1.$$

It follows at once from Euclid's point of view that no primes contained in any term up to u_x can appear in u_{x+1}, so that all the terms must be relatively prime to each other. The same consequence follows *a posteriori* from the scale of relation above given; for, as I had occasion to observe in the *Comptes rendus* for April 1888, if dealing only with rational integer polynomials,

$$\phi(x) = (x - a)f(x) + a,$$

then, whatever value, c, we give to x, no two forms $\phi^i(c)$, $\phi^j(c)$ can have any common measure not contained in a: in this case $\phi(x) = (x - 1)x + 1$; so that $\phi^i(c)$ and $\phi^j(c)$ must be relative primes for all values of i and j.[2]

It is worthy of remark that all the primes, other than 3, implicitly obtained by this process will be of the form $6i + 1$.

Euclid's own process, or the modified and less transcendental one, may be applied in like manner to obtain a continual succession of primes of the form $4n + 3$ and $6n + 5$.

[1] Whereas the English elementary book writers content themselves with showing that to suppose the number of primes finite involves an absurdity, Euclid shows how from any given prime or primes to generate an infinite succession of primes.

[2] Another theorem of a similar kind is that, whatever integer polynomial $\phi(x)$ may be, if i, j have for their greatest common measure k, then $\phi^k[\phi(0)]$ will be the greatest common measure of $\phi^i[\phi(0)]$, $\phi^j[\phi(0)]$.

As regards the former, we may use the formula

$$2 \cdot a \cdot b \ldots c + 1$$

(where $a, b, \ldots c$ are any "proposed" primes of the form $4n + 3$), which will necessarily be of the form $4n + 3$, and must therefore contain *one* factor at least of that form.

As regards the latter, we may employ the formula

$$3 \cdot a \cdot b \ldots c + 2$$

(where $a, b, \ldots c$ are each of the form $6n + 5$), which will necessarily itself be, and therefore contain *one* factor at least, of that form.

The scale of relation in the first of these cases will be, as before,

$$u_{x+1} = u_x^2 - u_x + 1;$$

so that each term in the progression, abstracting 3, will be of the form $4i + 3$ and $6j + 1$ conjointly, and consequently of the form $12n + 7$; as *e.g.*,

$$3,\ 7,\ 43,\ 1807,\ \ldots$$

In the latter case the scale of relation is

$$u_{x+1} = u_x^2 - 2u_x + 2,$$

which is of the form $(u_x - 2)u_x + 2$. It is obvious that in each progression at each step one new prime will be generated, and thus the number of ascertained primes of the given form go on indefinitely increasing, as also might be deduced *a posteriori* by aid of the general formula above referred to from the scale of relation applicable to each. Each term in the second case (the term 3, if it appears, excepted) will be simultaneously of the form $6i - 1$ and $4j + 1$, and consequently of the form $12n + 5$, as in the example 5, 17, 257, 65537, ...

The same simple considerations *cease* to apply to the genesis of primes of the forms $4n + 1$, $6n + 1$. We may indeed apply to them the formulæ

$$(2 \cdot a \cdot b \ldots c)^2 + 1 \text{ and } 3(a \cdot b \ldots c)^2 + 1$$

respectively, but then we have to draw upon the theory of quadratic forms in order to learn that their divisors are of the form $4n + 1$ and $6n + 1$ respectively.

Of course the difference in their favour is that in their case *all* the divisors locked up in the successive terms of the two progressions respectively are of the prescribed form; whereas in the other two progressions, whose theory admits of so much simpler treatment, we can only be assured of the presence of *one* such factor in each of the several terms.

Euler has given the values of two infinite products, without any evidence of their truth except such as according to the lax method of dealing with series without regard to the laws of convergence prevalent in his day, and still held in honour in Cambridge down to the times of Peacock, De Morgan, and Herschel inclusive (and this long after Abel had justly denounced the use of divergent series as a crime against reason), was erroneously supposed to amount to a proof, from which the same consequences may be derived as shown in the foregoing pages, and something more besides.[1] These two theorems are—

$$(1)\quad \frac{3}{3+1} \cdot \frac{5}{5-1} \cdot \frac{7}{7+1} \cdot \frac{11}{11+1} \cdot \frac{13}{13-1} \ldots = \frac{\pi}{4}$$

(where, corresponding to the primes 3, 7, 11, ... of the form $4n + 3$, the factors of the product on the left are

$$\frac{3}{3+1},\ \frac{7}{7+1},\ \frac{11}{11+1},\ \ldots$$

all of them with the sign + in the denominator; while the fractions corresponding to primes of the form $4n + 1$ have the − sign in their denominators).

$$(2)\quad \frac{5}{5+1} \cdot \frac{7}{7-1} \cdot \frac{11}{11+1} \cdot \frac{13}{13-1} \cdot \frac{17}{17+1} \ldots = \frac{\pi}{2}\sqrt{3}$$

where, as in the previous product, the sign in the denominator of each fraction depends on the form of the prime to which it corresponds (being + for primes of the form $6n - 1$, and − for primes of the form $6n + 1$).

[1] It follows from the first of these theorems that with the understanding that no denominator is to exceed n (an indefinitely great number), $(1 + \frac{1}{3})(1 + \frac{1}{7})(1 + \frac{1}{11})(1 + \frac{1}{19}) \ldots$ bears a finite ratio to $(1 + \frac{1}{5})(1 + \frac{1}{13})(1 + \frac{1}{17}) \ldots$ so that as their *product* is known to be infinite, each of these two partial products must be separately infinite; in like manner from Euler's second theorem a similar conclusion may be inferred in regard to each of the two products $(1 + \frac{1}{5})(1 + \frac{1}{11})(1 + \frac{1}{17})(1 + \frac{1}{23})(1 + \frac{1}{29})(1 + \frac{1}{41}) \ldots$ and $(1 + \frac{1}{7})(1 + \frac{1}{13})(1 + \frac{1}{19})(1 + \frac{1}{31}) \ldots$

Dr. J. P. Gram (*Mémoires de l'Académie Royale de Copenhague*, 6me. série, vol. ii. p. 191) refers to a paper by Mertens ("Ein Beitrag zur analytischen Zahlentheorie," *Borchardt's Journal*, Bd. 78), as one in which the truth of the first of the two theorems is demonstrated—"fuldstoendigt Bevis af Mertens" are Gram's words.[1]

Assuming this to be the case, we shall easily find when N is indefinitely great, so that S_N becomes $\frac{\pi}{4}$,

$$Q_N S_N = \frac{1}{(1-\frac{1}{3})(1-\frac{1}{5})\ldots\ldots\left(1-\frac{1}{N}\right)},$$

which, according to Legendre's empirical law (Legendre, "Théorie des Nombres," 3rd edition, vol. ii. p. 67, art. 397), is equal to $\frac{2\log N}{K}$, where $K = 1{\cdot}104$; and as we have written $Q_N S_N = \log N + (V - R)$, we may deduce, upon the above assumptions,

$$V - R = \left(\frac{2}{K} - 1\right)\log N = 0{\cdot}811\ldots\ldots\log N.$$

R, we know, is demonstrably less than $\left(1 - \frac{\pi}{4}\right)\log N$, consequently V must be less than $(0{\cdot}812 + 0{\cdot}215)\log N$, *i.e.* less than $1{\cdot}027\log N$, and *a fortiori* the portion of the omnipositive aggregate Q_N, which consists of terms whose denominators exceed N, when N is indefinitely great, cannot be less than $\frac{4}{\pi}\left(1-\frac{\pi}{4}\right)\log N$, *i.e.* $0{\cdot}273\log N$.

Before concluding, let me add a word on Legendre's empirical formula for the value of

$$(1-\tfrac{1}{3})(1-\tfrac{1}{5})\ldots\ldots\left(1-\frac{1}{p_{N,p}}\right),$$

referred to in the early part of this article.

If N is any odd number, the condition of its being a prime number is that when divided by any odd prime less than its own square root, it shall not leave a remainder zero. Now if N (an unknown odd number) is divided by p, its remainder is equally likely to be 0, 1, 2, 3, ... or $(p-1)$. Hence the chance that it is not divisible by p is $\left(1-\frac{1}{p}\right)$, and, if we were at liberty to regard the like thing happening or not for any two values of p within the stated limit as independent events, the expectation of N being a prime number would be represented by

$$(1-\tfrac{1}{3})(1-\tfrac{1}{5})(1-\tfrac{1}{7})(1-\tfrac{1}{11})\ldots\ldots\left(1-\frac{1}{p_{N^{\frac12},p}}\right),$$

which, according to the formula referred to, for infinitely large values of N is equal to $\frac{1{\cdot}104}{\log N^{\frac12}}$. It is rather more convenient to regard N as entirely unknown instead of being given as odd, on which supposition the chance of its being a prime would be $\frac{1{\cdot}104}{2\log N^{\frac12}}$ or $\frac{1{\cdot}104}{\log N}$.

Hence for very large values of N the sum of the logarithms of all the primes inferior to N might be expected to be something like $(1{\cdot}104)$N. This does not contravene Tchebycheff's formula (Serret, "Cours d'Algèbre Supérieure," 4me ed., vol. ii. p. 233), which gives for the limits of this sum AN and BN, where $A = 0{\cdot}921292$, and $B = \frac{6A}{5} = 1{\cdot}10555$; but does contravene the narrower limits given by my advance upon Tchebycheff's method (see *Am. Math. Journal*, vol. iv. Part 3), according to which for A, B, we may write A_1, B_1, where

$$A_1 = 0{\cdot}921423,\quad B_1 = 1{\cdot}076577.[2]$$

That the method of probabilities *may* sometimes be successfully applied to questions concerning prime numbers I have shown reason for believing in the two tables published by me in the *Philosophical Magazine* for 1883.[3]

New College, June 10. J. J. SYLVESTER.

[1] It always seems to me absurd to speak of a complete proof, or of a theorem being rigorously demonstrated. An incomplete proof is no proof, and a mathematical truth not rigorously demonstrated is not demonstrated at all. I do not mean to deny that there are mathematical truths, morally certain, which defy and will probably to the end of time continue to defy proof, as, *e.g.*, that every indecomposable integer polynomial function must represent an infinitude of primes. I have sometimes thought that the profound mystery which envelops our conceptions relative to prime numbers depends upon the limitation of our faculties in regard to time, which like space may be in its essence poly-dimensional, and that this and such sort of truths would become self-evident to a being whose mode of perception is according to *superficially* as distinguished from our own limitation to *linearly* extended time.

[2] Viz. $A_1 = \frac{51072}{50999}A$, and $B_1 = \frac{59595}{50999}A$, the values of which are incorrectly stated in the memoir. Strange to say, Dr. Gram, in his prize essay, previously quoted, on the number of prime numbers under a given limit, has omitted all reference to this paper in his bibliographical summary of the subject, which is only to be accounted for by its having escaped his notice; a narrowing of the asymptotic limits assigned to the sum of the logarithms of the prime numbers series being the most notable fact in the history of the subject since the publication of Tchebycheff's memoir. Subjectively, this paper has a peculiar claim upon the regard of its author, for it was his meditation upon the two simultaneous difference-equations which occur in it that formed the starting-point, or incunabulum, of that new and boundless world of thought to which he has given the name of Universal Algebra. But, apart from this, that the superior limit given by Tchebycheff as $1{\cdot}1055$ should be brought down by a more stringent solution of his own inequalities to only $1{\cdot}076577$—in other words, that the excess above the probable mean value (unity) should be reduced to little more than ⅔rds of its original amount—is in itself a surprising fact. Perhaps the numerous (or innumerable) misprints and arithmetical miscalculations which disfigure the paper may help to account for the singular neglect which it has experienced. It will be noticed that the mean of the limits of Tchebycheff is $1{\cdot}01342$, the mean of the new limits being $0{\cdot}99900$. The excess in the one case above and the defect in the other below the probable true mean are respectively $0{\cdot}01342$ and $0{\cdot}00100$.

[3] A principle precisely similar to that employed above if applied to determining the number of reduced proper fractions whose denominators do not exceed a given number n, leads to a correct result. The expectation of two numbers being prime to each other will be the product of the expectations of their not being each divisible by any the same prime number. But the probability of one of them being divisible by i is $\frac{1}{i}$, and therefore of two of them being not each divisible by i is $\frac{1}{i^2}$. Hence the probability of their having no common factor is

$$(1-\tfrac{1}{4})(1-\tfrac{1}{9})\ 1-\tfrac{1}{25})(\tfrac{1}{121})\ldots\ldots\ ad\ inf.,\ i.e.\ \text{is}\ \frac{6}{\pi^2}.$$

If, then, we take two sets of numbers, each limited to n, the probable number of relatively prime combinations of each of one set with each of the other should be $\frac{6n^2}{\pi^2}$, and the number of reduced proper fractions whose denominators do not exceed n should be the half of this or $\frac{3n^2}{\pi^2}$. I believe M. Césaro has claimed the prior publication of this mode of reasoning, to which he is heartily welcome. The number of these fractions is the same thing as the sum of the *totients* of all numbers not exceeding n. In the *Philosophical Magazine* for 1883 (vol. xv. p. 251), a table of these sums of totients has been published by me for all values of n not exceeding 500, and in the same year (vol. xvi. p. 231) the table was extended to values of n not exceeding 1000 In every case without any exception the estimated value of this totient sum is found to be intermediate between

$$\frac{3n^2}{\pi^2}\ \text{and}\ \frac{3(n+1)^2}{\pi^2}.$$

Calling the totient sum to n, T(n), I stated the exact equation

$$T(n) + T\left(\frac{n}{2}\right) + T\left(\frac{n}{3}\right) + T\left(\frac{n}{4}\right) + \ldots\ldots = \frac{n^2+n}{2},$$

from which it is capable of proof, without making any assumption as to the form of Tn, that its asymptotic value is $\frac{3n^2}{\pi^2}$. The functional equation itself is merely an integration (so to say) of the well-known theorem that any number is equal to the sum of the totients of its several divisors. The introduction to these tables will be found very suggestive, and besides contains an interesting bibliography of the subject of Farey series (*suites de Farey*), comprising, among other writers upon it, the names of Cauchy, Glaisher, and Sir G. Airy, the last-named as author of a paper on toothed wheels, published, I believe, in the "Selected Papers" of the Institute of Mechanical Engineers. The last word on the subject, as far as I am aware, forms one of the *interludes*, or rather the *postscript*, to my "Constructive Theory of Partitions," published in the *American Journal of Mathematics*.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 3.—"Electro-Chemical Effects on Magnetizing Iron." Part II. By Thomas Andrews, F.R.S.E. Communicated by Prof. G. G. Stokes, P.R.S.

The present paper contains the results of a further study of the electro-chemical effects observed between a magnetized and an unmagnetized bar when in circuit in certain electrolytes, recorded in Part I. of this research. The method of experimentation was generally similar to that pursued and described in Part I.,

though it was necessary to introduce numerous modifications of detail and also new modes of experimentation. The bars experimented on were of specially prepared wrought-iron and cast-steel; all the rods were finely polished, and the general physical properties of the metals are given in Table B. Steel bars were employed in some of the experiments, because after magnetization by the coil their subsequent influence as permanent magnets could be observed. The reagents employed as electrolytes consisted of various solutions of bromine, ferric chloride, and chlorine water, ferrous sulphate, ferric chloride, cupric chloride, cupric sulphate, cupric nitrate, cupric acetate, cupric bromide, nickel chloride, hydrochloric, acid, nitric acid, and potassium chlorate. A pair of bars in each experiment were immersed as elements in the solution in the special apparatus employed, in circuit also with a delicate galvanometer, and after normal galvanic equilibrium had been obtained the bar within the coil was magnetized for various periods and the magneto-chemical effect observed. It was found to vary with the nature of the metal and solution employed, and also with the extent of the magnetization of the metals. The average results of many repeated experiments are given in numerous detailed tables, and it was generally found that a magnetized bar became electro-positive to an unmagnetized one. In Parts I. and II. a total of near 600 iron and steel bars have been experimented upon. Experiments were also made showing that local currents were developed in a magnetized bar between the more highly and less magnetized parts thereof, when the rod was immersed in suitable solutions acting chemically upon it.

Interesting experiments have also been made in connection with the influence of magnetization on the action of strong nitric acid on iron and steel. In course of the research the results of an extensive quantitative study of magneto-chemical phenomena have been recorded, the effect in connection with a considerable variety of typical reagents having been carefully observed; with some reagents the effect was found to be comparatively small, in other instances it was somewhat considerable. The general conclusion was that under the conditions recorded a magnetized bar was electro-positive to an unmagnetized one, when the two were immersed in suitable solutions, and that the extent of the result was in some degree dependent both on the nature and strength of the solution, and also on the extent of the magnetization of the metal.

June 7.—"Note on the Volumetric Determination of Uric Acid." By A. M. Gossage, B.A. Oxon.

It seemed improbable that the method recently proposed by Dr. Haycraft for the volumetric determination of uric acid in urine could be accurate, since both Salkowski and Maly had previously shown that the precipitate of silver urate obtained from urine contains variable quantities of other urates. To test the method, I examined samples of various urines both by his method and by that of Salkowski, which is universally acknowledged to be the most trustworthy. The mean percentages of uric acid found were as follow:—

Experiment	I.	II.	III.	IV.	V.
Haycraft's method	0·108	0·076	0·082	0·072	0·108
Salkowski's method	0·084	0·035	0·051	0·035	0·084

The results obtained by Haycraft's method were always considerably higher than those obtained by Salkowski's. The reason of this is that Dr. Haycraft has assumed that the silver precipitate from urine consists of a urate containing only 1 atom of silver in the molecule, whereas the proportion of silver in silver urate corresponds more nearly to 2 atoms in the molecule. Assuming, then, that there are 2 atoms of silver in all the molecules of the urate, and dividing the results obtained by Haycraft's method by two, we see that the results so obtained are usually lower than those obtained by Salkowski's method, and that the proportion between the results by the two methods varies, as would be expected from Salkowski's researches.

EDINBURGH.

Royal Society, June 4.—Dr. John Murray, Vice-President, in the chair.—Dr. G. Sims Woodhead exhibited a series of photographs of large sections of the lung.—A paper by the Astronomer-Royal for Scotland on Scottish meteorology for the last thirty-two years was read.—Dr. E. Sang read a paper on John Leslie's computation of the ratio of the diameter to the circumference of a circle.—A paper by Lord Maclaren on the figure of aplanatic lenses was read.—Prof. Tait submitted some quaternion notes.

June 18.—The Hon. Lord Maclaren, Vice-President, in the chair.—The Secretary exhibited M. Amagat's photographs of the crystallization of chloride of carbon under pressure alone.—A paper by Prof. W. Carmichael McIntosh and Mr. E. E. Prince, St. Andrews' Marine Laboratory, was communicated.—A paper by Prof. Anglin on certain theorems mainly connected with alternants, was read.—Prof. Haycraft and Dr. R. T. Williamson gave a demonstration of a method, which can be used chemically, for estimating quantitatively the alkalinity of the blood.—A preliminary notice of a paper by Dr. G. N. Stewart on electrolytic decomposition of proteid substances was submitted.—Papers by Dr. A. B. Griffiths, on the Malpighian tubules of *Libellula depressa*, and on a fungoid disease in the roots of *Cucumis sativa*, were communicated.

PARIS.

Academy of Sciences, July 2.—M. Janssen, President, in the chair.—Reply to Mr. Douglas Archibald's strictures on the subject of storms, by M. H. Faye. The storm laws, as established by the observations of Capper, Piddington, Reid, and Redfield, are declared to be one of the greatest discoveries of the century, and their truth is here vindicated against the recent attacks of Prof. Loomis, Dr. Meldrum, and especially Mr. E. Douglas Archibald, in NATURE for June 14 (p. 149). Archibald's diagram of the Manilla cyclone of October 20, 1882, is here reproduced, and it is contended that these highly characteristic phenomena can be explained only by admitting a *descending* motion in the central part of the cyclone. But on the opposite supposition it is precisely here that the *ascending* current should be strongest, for this central region corresponds exactly to the minimum of barometric pressure. The error in this theory of his opponents is attributed to a confusion between two quite distinct kinds of depressions, a confusion which has for fifty years impeded the progress of meteorological science and increased the perils of navigation.—On the cultivation of Bœmaria in Provence, by M. Naudin. The author reports that the white species (*B. nivea*), lately introduced from China, thrives well in the Antibes district, where the green variety (*B. utilis*) has long been acclimatized. The foliage makes excellent fodder for cattle.—Automatic control of the velocity in machinery of variable action, by M. H. Léauté. An apparatus, the result of many years' study, is here described, by means of which the action of engines may easily be regulated, even when required to work at varying rates of speed.—On a compass enabling the observer to find the meridian on land or water despite the disturbing influence of iron, by M. Bisson. An ingenious apparatus is described by means of which the compass may be prevented from deviating more than one-tenth of a millimetre, even in the neighbourhood of iron. It has been tested with satisfactory results on board several French ironclads, and works equally well by land or sea.—On the snows, ice, and waters of Mars, by M. Flammarion. In reply to some recent remarks on the meteorological condition of this planet, it is pointed out that the varying state of the polar ice-caps has long been carefully observed by Maedler, Schiaparelli, and others, the inference being that Mars is not in a state of glaciation. On the contrary its temperature is equal to, if not higher, than that of the earth, and its polar snows melt periodically to a far greater extent than on our planet.—On the graphic representation of numerical divisors, by M. Saint-Loup. By adopting a rectangular distribution of the numerals, the author arrives at some practical results on the general grouping of the prime numbers.—On the determination of the constants and of the dynamic coefficient of elasticity for steel, by M. E. Mercadier. By the method already indicated (*Comptes rendus*, July and August, 1887), the author here determines the relation $\frac{\lambda}{\mu}$ of the constants for steel. In a future paper will be given the results of the experiments undertaken to determine the coefficients of electricity.—On the mechanism of electrolysis by the process of alternative currents, by MM. J. Chappuis and G. Maneuvrier. The recognized impossibility of electrolysing the sulphate of copper by alternative currents is explained by the theory that the copper deposited on each electrode by one of the currents is immediately dissipated by the inverse current. This explanation is here justified by the authors' experiments, which render *visible* the decomposition of the sulphate of copper, as they had previously done for acidulated water. From this experimental study they hope to deduce the general principles for the prac-

tical application of alternative currents in the process of electrolysis.—Application of Carnot's principle to endothermic reactions, by M. Pellat. By distinguishing between the temperature of the bodies giving rise to the endothermic reaction and that of the source supplying in the form of heat the energy needed for the reaction, the author is led by the application of Carnot's principle to a law analogous to that of Potier, but of a more general character. — On the hydrochlorate of cupric chloride, by M. Paul Sabatier. The author admits the priority of M. Engel's researches on the properties and preparation of this substance, but points out that this chemist gives it a very different composition from that which he has himself obtained, and which is represented by the formula CuCl, HCl, 5HO.—On the artificial reproduction of the micas and of scapolite, by M. Doelter. A process is described, by means of which the author has artificially reproduced the chief minerals of the mica group, as well as of natural scapolite. He has already effected the synthesis of biotite, phlogopite, muscovite, and lepidolite (zinnwaldite variety).—Fresh physiological researches on the organic substance which has the property of hydrogenating sulphur, by M. J. de Rey-Pailhade. During his further study of this substance, to which he has given the name of philothion, the author has determined several new facts, amongst others that when the yeast is treated by reagents, the death of the organism always precedes the destruction of this organic substance. Philothion is generated by the physiological development of the yeast, and combines with sulphur according to an equation of which sulphuretted hydrogen is a factor. Acting as a diastase, it adds a fresh proof to M. Berthelot's theory of fermentation. Lastly, it is the first known instance of a substance extracted from a living organism which has the property of hydrogenizing sulphur.—Prof. Langley has been elected by a large majority to succeed the late M. Roche as Corresponding Member of the Academy on the Section of Astronomy.

BERLIN.

Physiological Society, June 22.—Prof. du Bois Reymond, President, in the chair.—Dr. H. Virchow spoke on the blood-vessels of the eye in Carnivora as worked at by Bellarminoff under his direction. The communication was illustrated by drawings and the exhibition of preparations. The points of most general interest which stand out from among the mass of details in this research are that the blood-vessels of the eye have a tendency to form rings from which a large number of fine branches pass posteriorly; further that the arrangement is often very different in different classes of animals, thus, for instance, the course of the arteries in the eye of a dog as compared with that of a rabbit is such that the dog's eye must be turned through an angle of 180° in order to make the course of its arteries correspond with that of the rabbit's eye.—Dr. Heymans communicated the results of his researches on the nerve-endings in the unstriated muscle-fibres of the medicinal leech. In the alimentary canal of the Hirudinea the muscle-fibres are placed both longitudinally and circularly; they consist of a contractile sheath and a protoplasmic axis containing the nucleus, and either have pointed ends or else divide into two or more branches, each of which then ends in a point. The muscle-fibres are separated from each other by large interstitial spaces filled with connective tissue, in which the nerve-plexus lies and sends fine nerve-branches into the muscle-fibres. The nerves end partly as extremely fine filaments and partly as round, flattened end-plates, and in no case does the nerve-ending penetrate the contractile sheath of the fibre so as to come into connection with the protoplasmic axis. In the vascular system of the leech the muscular layers are principally disposed in a circular fashion, but frequently the speaker noticed that at some point or another a circular fibre divided itself into two branches, and that the latter were then bent through a right angle so as now to pass in a longitudinal course in the wall of the blood-vessel. The nerve-endings in the fibres of the vascular system are the same as in those of the alimentary canal. Similarly, the muscle-fibres in the vascular system do not lie in close apposition to each other, but are separated by interstitial spaces; each fibre also contains only one nucleus.—Dr. van der Gehnchten, of Holland, gave a short abstract of his observations on the minute structure of striated muscles in Vertebrata and Arthropoda. He described the appearance of the muscles in the fresh conditions, after the coagulation of the myosin and after the solution of the amorphous proteid, and illustrated his statements by drawings. According to these researches the muscle-fibre of the Vertebrata consists of a network of doubly-refractive filaments, whose meshes are filled with the semi-fluid plasmatic substance. In Arthropoda the structure differs according as the muscle is taken from the wings or the legs; when taken from the latter the structure is extremely similar to that in the Vertebrata. In the discussion which followed, Dr. Benda pointed out that being engaged for years in studying the structure of striated muscle he had often obtained preparations similar in appearance to those of Dr. van der Gehnchten, but his interpretation of these appearances was very different. He pointed out, moreover, that he had often observed transitional forms between the muscles of the leg and wing in Arthropoda and those of Vertebrata. Without entering into any details, Dr. Benda gave it as his opinion that the network in a striated muscle-fibre must not be regarded as contractile, but as a connective-tissue interstitial substance, in whose interspaces the really contractile muscle fibrillæ lie.

IN the report of the meeting of the Physical Society in NATURE of June 21, p. 192, for "Dr. Lummer" (line 37 from the bottom) read "Prof. von Helmholtz."

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Geologische Studien ueber Niederlandische West Indien, 2te. Lief. Holländisch Guyana, K. Martin (Brill, Leyden).—Lectures on Geography: Lieut.-General R. Strachey (Macmillan).—British Dogs, No. 21: H. Dalziel (Gill).—Speaking Parrots, Part 3: Dr. K. Russ (Gill).—India in 1887: Prof. R. Wallace (Oliver and Boyd).—Annual Report of the Aëronautical Society of Great Britain for the years 1885–86 (Hamilton).—Beiblätter zu den Annalen der Physik und Chemie, 1886, No. 6 (Leipzig).—Geological Magazine, July (Trübner).—Journal of Anatomy and Physiology, July (Williams and Norgate).—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, Jahrg. v., 1886 (Magdeburg).—Zeitschrift für Wissenschaftliche Zoologie, xlvi. Band, 4 Heft. (Leipzig).—Mind, July (Williams and Norgate).—Notes from the Leyden Museum, vol. x. No. 3 (Leyden).—Journal of the Chemical Society, July (Gurney and Jackson).

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